

PROCEEDINGS  
OF THE  
ROYAL SOCIETY OF EDINBURGH.

VOL. XV.

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No. 126.

THE 105TH SESSION.  
GENERAL STATUTORY MEETING.

*Monday, 28th November 1887.*

The following Council were elected :—

*President.*

SIR WILLIAM THOMSON, F.R.S.

*Vice-Presidents.*

DAVID MILNE HOME, Esq. of Milne-  
Graden, LL.D.  
JOHN MURRAY, Esq., Ph.D.  
Professor Sir DOUGLAS MACLAGAN.

The Hon. Lord MACLAREN, LL.D.  
Rev. Professor FLINT, D.D.  
Professor CHRYSTAL, LL.D.

*General Secretary*—Professor TAIT.

*Secretaries to Ordinary Meetings.*

Professor Sir W. TURNER, F.R.S.  
Professor CRUM BROWN, F.R.S.

*Treasurer*—ADAM GILLIES SMITH, Esq., C.A.

*Curator of Library and Museum*—ALEXANDER BUCHAN, Esq., M.A., LL.D.

*Ordinary Members of Council.*

Professor BUTCHER, LL.D.  
Professor M'KENDRICK, F.R.S.  
THOMAS MUIR, Esq., LL.D.  
Professor M'INTOSH, F.R.S.  
Sir ARTHUR MITCHELL, K.C.B., LL.D.  
STAIR AGNEW, Esq., C.B.  
R. M. FERGUSON, Esq., Ph.D.

A. FORBES IRVINE, Esq. of Drum,  
LL.D.  
Dr J. BATTY-TUKE, F.R.C.P.  
Professor BOWER.  
Dr G. SIMS WOODHEAD, F.R.C.P.  
ROBERT COX, Esq. of Gorgie, M.A.

By a Resolution of the Society (19th January 1880), the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council :—

HIS GRACE THE DUKE OF ARGYLL, K.T., D.C.L.  
THE RIGHT HON. LORD MONCREIFF of Tulliebole, LL.D.

*Monday, 5th December 1887.*

The HON. LORD M'LAREN, Vice-President, in  
the Chair.

**1. Chairman's Opening Address.**

In commencing the business of a new Session of the Royal Society, it is natural to refer to the work of the Society in the year which has been completed. A Society such as this is, constituted for the promotion of literary and scientific research, works in two ways, and first and chiefly by the reading and publication of papers either extending the boundaries of scientific acquirement, or recording the finished results of observation and experiment which are the foundations of theoretical research. To this work only a limited number of our fellows are able to contribute, but it is to be hoped that such of our number as do not contribute to the Society's publications may at least by their presence, and the interest which they manifest in the subjects of the papers, do something to encourage the more active members of the Society in the work of research.

But, secondly, it is part of the proper work of this Society, by its organisation, its influence, and the expenditure of its funds, to aid the work of research in these departments, in which results cannot be achieved by the unaided exertions of individual members.

I shall not attempt to enter into particulars regarding the progress of scientific research during the current year. To a proper estimate of its results it would be necessary that a report should be prepared by a combination of men who are themselves engaged in mathematical, physical, and biological investigation, and who are acquainted with the work done by specialists in their respective sciences.

But it may be interesting to the members to hear something regarding the business of the Society as an organisation working for public objects, and I shall endeavour to notice briefly the work in which the Council has been recently engaged. The condition of the Society itself, I am happy to say, is entirely satisfactory.

Indeed, so far as I know, the only difficulty with which the Council has had to contend is the accumulation of literary material in our Library. Our shelves, as you see, are filled with books and



transactions from floor to ceiling, and we have nearly come to the point when we must choose between the sacrifice of a part of our collection, and the alternative of providing additional library accommodation.

The Council has been giving its attention to this subject, and I am confident that any practical suggestions which the Council may hereafter be able to bring before us will have the support of the Royal Society.

You are aware that our library, I may say like everything that is undertaken by this Society, is distinctly specialised. The object of the Council has been to make it a complete collection of original memoirs on every scientific subject. It includes the transactions of every scientific society of repute throughout the world, and all that are really valuable among the scientific periodicals of our own and foreign countries. Members who are working on special subjects accordingly have at their disposal the original papers in which the discoveries connected with these subjects were communicated to the public, and where the details of investigations are more fully given than in the text-books in which the results of research are put together. This is obviously the most useful kind of reference library for a Society such as ours; and on the authority of our Librarian, I may state that our library is the most complete collection of the kind in the United Kingdom, more complete even than that of the Royal Society of London.

In making this statement, I am not at all afraid that some section of the community will be seized with a desire to appropriate our collection for the purposes of a public library. I am inclined to think that our literary treasures will be found for the most part capable of exerting a remarkable force of repulsion upon any one who approaches them without due preparation. They are eminently useful to students of science, and not particularly interesting to the general reader. Such being its character, it is desirable that our library should be well cared for, well arranged, and made accessible to members.

The Council having found that it was impossible that the duties of an assistant librarian and assistant secretary could be performed single-handed, resolved last summer to engage a second assistant librarian, to take a share of the duties which had hitherto fallen to

Mr Gordon, and I think that members have every reason to be satisfied with the manner in which these duties are discharged.

One of the events of the year which calls for more than a passing notice, is the institution of the Victoria Jubilee Prize by Dr Gunning, a gentleman distinguished by his enlightened liberality, and who has given away a large part of his fortune to the enrichment of the institutions of this city.

By the terms of the foundation, the prize is to be awarded triennially, and may be either for work done during the past three years, or for researches to be prosecuted during the ensuing triennial period on a subject approved by the Council.

In calling the attention of the Society to the special terms of Dr Gunning's most useful endowment, I note that this prize fund is, so far as I know, the only fund available for distribution in Scotland in aid of scientific research. There is a research fund, as the Fellows are aware, voted annually by Parliament, and placed at the disposal of a committee of the Royal Society of London; but no share of this or any other public fund is placed at the disposal of the scientific profession in Scotland.

Ireland has its special grant; but Scotland has to trust to the liberality, or let me say the sense of justice, of the Royal Society of London in regard to such claims as she may prefer for the work of scientific research in Scotland. We know that when a demand comes from a Scotsman of established reputation in the scientific profession, it is fairly considered by the committee in London, and generally honoured. I need only refer to the sum so liberally voted at the last meeting of the London Committee in aid of Dr Murray's submarine researches, and, indeed, it is not to be doubted that the eminent men who compose the Research Committee in London are anxious to make a fair distribution of the public money which is placed at their disposal.

But we say that it is impossible in the nature of things that a committee in London should have the knowledge that is desirable of the attainments of the younger scientific men of Scotland who may be willing to engage in original work.

The work proposed may be most important, and its prosecution may involve the purchase of costly apparatus, such as would be a proper charge on the research fund; but how is the committee

sitting in London to find out whether the applicant is qualified by training and experience to prosecute the researches which he proposes to undertake? It is plain enough that a committee must to a large extent act on its personal knowledge of the applicants, and that the unknown applicant for a share of the research fund has a tolerable chance of being defeated in that struggle for existence which is illustrated by scientific competition.

It is, therefore, very necessary that the distribution of research funds should be in some degree localised. Should any Fellow of this Society be disposed to carry on the work initiated by Dr Gunning, we can assure him that, as to any superfluous money, the possession of which may be useless or injurious to himself, he cannot employ it more usefully than by aiding in the establishment of a research fund, to be distributed by a committee of the scientific body in Scotland.

We also invite the public of Scotland, and its representatives in Parliament, to support the Royal Society of Edinburgh in its claim to have a share of the Government grant allocated to Scotland for researches conducted within the country.

I have now to allude to a cognate subject, which at the present moment possesses a special interest and importance: I refer to the Ben Nevis Meteorological Observatory.

I remember to have seen somewhere an engraving after one of the old masters, in which Science is represented as a melancholy female form perched upon a hill and surrounded by circles, instruments of alchemy, and other quaint devices. I think that the mediæval artist must have had what we call a forecast of those mountain establishments which have sprung up in different parts of the world, and among which the Observatory at Ben Nevis is the most famed for its admirable situation, and the high character of the work which is there performed. Its hourly observations, taken without intermission during a period of three years by Mr Omond and his assistants, have now been reduced and tabulated. They are in print, and will shortly be issued as an extra number of the *Transactions* of this Society.

It is unnecessary to point out that such a series of observations must be of the highest value for general purposes, such as the determining the constants of atmospheric pressure and temperature in

relation to height. But this is not all. I believe it will be found, when these observations are discussed, that the variations of atmospheric pressure at this elevated station, uninfluenced by local causes, can be reduced to something approaching to a general law, and that the theory of storms or waves of depression and elevation in the atmosphere will receive valuable elucidation from the results of this new departure in meteorological observation.

Up to this time the Observatory at Ben Nevis has been supported exclusively by the voluntary contributions of persons interested in scientific research. Out of such contributions the Observatory has been built and equipped, and the salaries (inadequate as they are for such service) have been provided. There is reason to hope that, either by direct grant from the Government, or by the assignment of a part of the sum which is annually voted by Parliament for distribution by the English Meteorological Council, a part at least of our future expenditure may be met.

The Fellows will agree with me in congratulating Mr Buchan, and the Scottish Meteorological Society with which he is connected, on his nomination to the vacant seat in the English Meteorological Council. I think that Mr Buchan's appointment may be regarded as a recognition by very high authority of the value of the Scottish meteorological work; and it is to be hoped that our representative (if we may so consider him) will be able to give such information to his colleagues as will eventually lead to the establishment at Ben Nevis being made independent of personal contributions.

At the same time, it is necessary to add that the establishment is incomplete. A new room is wanted at the Observatory for the seismometers, and a sea-level station at Fort William is necessary for the purpose of proper comparison observations. The Scottish Meteorological Society has undertaken to provide these buildings on condition of being relieved of the future maintenance of the Observatory; and it is hoped that this expense may be in part met by a grant out of the surplus funds of the Edinburgh Exhibition, if the members of that Association shall see fit to accede to the application that has been addressed to them.

I am unwilling to bring these remarks to a close without making reference to a kindred institution, which has even a stronger claim than Ben Nevis on the liberality of the public Exchequer. The



Edinburgh Astronomical Observatory is a Government edifice, taken over by Government from a private society under an obligation to maintain it in perpetuity. It is understood that, in what relates to original scientific work, the operations of the Observatory have been to a considerable extent suspended in consequence of the want of the necessary instrumental means. The old instruments are worn out, and the new equatorial is incomplete. The English Government, which in matters relating to science acts spasmodically, and only under pressure, some ten years ago paid a large sum of money for the purchase of a handsome equatorial telescope for the Observatory at Edinburgh. During these ten years they have allowed their instrument to remain unproductive, apparently because they would not advance the few hundred pounds that are necessary to adapt the instrument to the special requirements of the work which it is to perform. I shall not enter into further detail in this matter, because the Astronomer-Royal is to read a paper this evening on the Edinburgh Equatorial in 1887.

But if the Edinburgh Observatory is to be kept up as a national institution,—and I trust it will remain such in accordance with the agreement between the founders and the Government of the day,—it must be adequately endowed and properly equipped. After the Royal Society has finished the work it has taken in hand in relation to Ben Nevis, I hope you will take up the subject of the Edinburgh Observatory, and that you will not let the matter rest until the Observatory has been restored to the position of a first-class observing station. It may be thought by some that it is not desirable to maintain at the public expense two such establishments—the one in London and the other in Edinburgh. Nor would it be desirable, if the two establishments were to be employed in doing the same kind of work. But there is a vast amount of skilled work to be done in Sidereal Astronomy, which can only be done in a public observatory, where, as a matter of duty, assistants are in attendance every night. The researches to be undertaken here need not be of the same description as the work undertaken at Greenwich. In proof of this assertion, I will only mention what is being done at the present time in the Government Observatory at the Cape, by that accomplished astronomer and indefatigable worker, Dr Gill.

First, one of the assistants has been employed during the last three years on a photographic survey of the southern heavens: not a survey on the colossal scale recommended by the Paris Conference, but on a more manageable and perhaps more useful scale—a scale sufficient for the requirements of the practical astronomer, and containing more stars than are contained in the maps of Argelander, which, as you know, are the best existing maps of the northern heavens. Each photographic picture of the Cape series covers an area of four square degrees, and about a half of the work is already done. The places of the stars are being reduced and tabulated by microscopic measurement.

Secondly, the Cape Observatory is engaged in a redetermination of the right ascensions of fundamental stars, by means of circle observations taken in the prime vertical; that is, in the great circle of the heavens which passes from west to east through the zenith. Hitherto right ascensions have been determined mainly by means of time observations, and their places are affected by errors due to the small irregularities of clocks, but still more by errors arising from the impossibility of estimating the one-tenth of a second by eye and ear. Under the Cape series of determinations the right ascensions of all the stars which do not set between south and west will be determined by circle readings with the same degree of precision as polar distances are now determined.

Thirdly, the Cape Observatory, as I learn by a communication from its director, has commenced a systematic investigation of the parallax of the southern stars. I should like to say something about the instrument—a masterpiece of mechanical and optical skill—with which these delicate measures are being made. But this would be wandering too far from my subject. What I wanted to say is, that we are not at the present time, either in Greenwich or elsewhere, doing, for the northern half, any one of these three things which three of the chief instruments of the Cape Observatory are employed in doing for the southern half of the celestial sphere. You see there is scope for division of labour in astronomy as in other things. It is really not doubtful that, with a proper equipment, our Observatory could be employed on work such as can only be done at a national observatory, under official rules and discipline, which should not in the least compete with the work done at the National Observatory of Greenwich. With the greatest respect for

the views of professional gentlemen near me, I venture to think that the Edinburgh Observatory could be made more useful by being maintained in its present position than by being made over to the University of Edinburgh, according to the Government proposals of last session. But whether it is to be kept up as a national or as an academic institution, let us urge on the Government the duty of making it efficient, by providing the astronomer with the necessary instrumental appliances.

I have now to open the business of the Session by presenting the Victoria Jubilee Prize to our President, Sir William Thomson.

## 2. The Jubilee Prize.

The Chairman, on presenting the Victoria Jubilee Prize to Sir William Thomson, said :—

The Council of this Society, at their meeting on the 15th of July last, approved of the following Report by their Committee:—The Committee appointed to recommend the first award of the Victoria Jubilee Prize, having taken into consideration the terms of the foundation of the Victoria Jubilee Prize, were of opinion that the Council, in making their first award, ought to give the prize for work already done, especially as the *Transactions* and *Proceedings* of the Society contain evidence of a large amount of valuable scientific work contributed by gentlemen connected with Scotland during the past three years.

Having gone over the list of papers during that period, and considered suggestions regarding the experimental work which had been in progress during the prescribed period, the Committee resolved to recommend to the Council that the prize should be awarded to Professor Sir William Thomson, P.R.S.E., for a remarkable series of papers on Hydrokinetics, especially on Waves and Vortices, which have been for some time, and are still being communicated to the Society.

Sir William Thomson has worked at Hydrokinetics almost since he began to publish.

Some remarkable papers of his on the Bounding Surface, and on the Vis-Viva of a moving liquid, appeared in the *Cambridge and Dublin Mathematical Journal*.

Helmholtz's great discovery of the properties of Vortex Motion attracted his special attention in 1867, and led him to the celebrated hypothesis of Vortex Atoms. His great paper on Vortices appeared in our *Transactions*, and several lesser, but important, ones in our *Proceedings*:—such as

On Maximum Energy in Vortex Motion.

On the Production of a Coreless Vortex.

Next we have his explanation of the apparent attractions and repulsions exerted by bodies vibrating in a fluid.

The effect of wind in raising waves.

Propagation of ripples by surface tension.

Motion of solids (with or without perforations) in a perfect liquid, when the motion is irrotational.

Stationary waves in running water.

Ring-waves produced by a single impulse.

Stability of Fluid Motion.

Laminar motion in a turbulently moving perfect liquid.

These form a collection of most important, and entirely novel, contributions to Hydrokinetics, which will bear comparison with the very best work ever done on the subject.

It is understood that a great part of this work has arisen indirectly from Sir W. Thomson's investigations as to the mechanism of the propagation of light, which have been given in outline in the Papyrograph of his Baltimore Lectures.

### 3. Proposed Additions to the List of Honorary Fellows.

The Chairman, in accordance with Law XII., read the following list:—

Ernest Haeckel, Professor of Zoology and Histology in the University of Jena.

Rudolph Julius Emmanuel Clausius, Professor of Natural Philosophy in the University of Bonn.

Demetrius Ivanovich Mendeléeff, Professor of Chemistry in the University of St Petersburg.

The following Communications were read:—



4. The *Edinburgh Equatorial* in 1887; a Paper with two Appendices. By C. Piazzì Smyth, *Astronomer-Royal for Scotland*.

The Equatorial of the Royal Observatory, Edinburgh, is still in October 1887, unfinished, blocked against use, and entirely unusable.

This lamentable outcome of so many years, is simply the consequence of the necessary funds for finishing and working the instrument having been withheld by Government, after they had been promised by the Board of Visitors, printed again and again in much detail, and thought to have been obtained.

The beginning, or first supplying, of the instrument had been carried out by H.M. Office of Works in London, on the strength of a special grant by Parliament; and the Astronomer knows simply nothing about the money part of that proceeding.

He has merely to do with what results it accomplished in its day;—and seeing that these are now freely confessed by the authorities actually concerned to be imperfect, and left standing, as well as locked and blocked, in that condition—to set forth from his point of view how,—assuming the working funds, as well as liberty to act, were to be granted to him, according to the original suggestion of the Board of Visitors,—he would set about the finishing of the instrument suitably with both its long ago accomplished beginning, the very peculiar artificial and natural restrictions of the situation, and the high nature of the stellar and spectroscopic observations required, and intended, to be made.

Taking the instrument therefore as it is now, or even with such minor improvements as a Government Commission (without the Astronomer upon it) recommended in 1879, and even obtained a grant for their execution by the Office of Works (though they have not been executed yet)—there is no doubt that if any experienced practical astronomer were to endeavour at this time to use the instrument, he would speedily arrive, amongst various other defects, at the following four accusations in chief, viz. :—

1st. The instrument, though not in the slightest degree too large for its intended work, and far smaller than many telescopes elsewhere, is yet too bulky for its Dome; and that has the largest

possible size allowed by the distinguished architectural adviser applied to by Government on the occasion of ordering it.

2nd. The instrument is too heavy and too severe in pressure for its bearings, compatibly with its quick and slow motions, and more especially for its delicate clock-movement.

3rd. The instrument is too awkward and multi-local as to its eye-pieces, handles, cords, finders, ladders, &c. &c.; and the observer far too much exposed in strained positions to the violence of the wind and intensity of the cold, to be likely to resist their influence long, or make very good observations at any time.

4th. The instrument is too weak in its spectroscope; and the latter too barbarous in its appliances, so far as they have yet been carried out.

To meet these evils the Astronomer suggests as follows; viz.—for Group No. 1, he proposes to shorten both the length of the telescope by its revolving head, and the length of its Declination axis by its outer 14 inches of excess far beyond its bearings; besides stripping off the great outside finder, the small outside finder, the long reading microscopes and a variety of other untoward excrescences; appropriate sub-arrangements being introduced to render these changes not only compatible with efficiency, but much more efficient, quick and handy.

For Group No. 2 he proposes to remove, with the revolving head, both the weights and the counter-weights of the spectroscope, heavy eye-piece plate, and second finder now at the upper long end of the tube; also the double counterpoise weights thereof at the lower short end of the tube; and then the triple counterpoise weights of the same at the end of the Declination axis,—thereby getting rid at once of more than 500 lbs. of dead weight, pressing at present with pernicious effect on the lower end of the Polar Axis, which is too small to bear much.

For Group No. 3, the Astronomer proposes, by a very simple yet radical change of eye-end arrangement, to have the eye-pieces of telescope, spectroscope and new finder, together with the slow-motion handles in R.A. and Decl., brought to, and arranged round, the end of the Declination Axis as already shortened; and where they will always be directly accessible to the observer at easy standing height on the floor and never exposed under the open shutter.

Also to exchange the present inefficient, yet cumbersome travelling and elevating platform and ladders, for a neat, compact, well seated and suitably fitted Observer's travelling hut,—freely traversing around, or to and from its generally proper and fully sheltered position at the end of the Declination axis, in any and every position of the telescope for observation arranged on the principles noted on the next page.

And for Group No. 4,—he finally proposes to construct a new and grand Spectroscope with two sets of prisms (after the manner of that which he made for himself in 1882, and therewith discovered the exquisite spectral progression of Carbonic oxide, as well as the compound triples of pure Oxygen, in gas vacuum tubes) occupying in one plane and chiefly in a diagonal direction therein, all the hitherto unoccupied length between the upper or small-mirror end of the telescope tube, and the outer end of the Declination axis (as shortened); thereby balancing in itself, across the Polar axis, the heavy telescope tube, and its very heavy lower, or great-mirror, end; and allowing an equivalent of dead weight to be taken off the Declination Axis.

While he proposes also to utilise the whole length of the telescope, and the axial dark space necessarily running up through it as a Newtonian (or in this case a semi-Newtonian) reflector,—first in the part above the small diagonal mirror, for the objective of a large centrally placed finder to the telescope, always looking fully out of the opened shutter, whenever the telescope itself does (while it sends its cone of rays, by a diagonal mirror of its own, down to the end of the declination axis); and then below it, for the collection of rays for an end-on, gas-vacuum and electric lighted tube of his own invention, to form the reference spectrum for stars, in a manner more unexceptionable it is believed and more promising for accuracy than any arrangement yet in use elsewhere. The observations being always as a rule,—and a rule most essential in the midst of a great and smoky city, growing greater and smokier day by day,—confined to as near the Meridian, and to as high an altitude therein, as possible.

And now, as I believe that the above suggestions, worked out already to sufficient extent on paper, meet all the difficulties yet found with even superfluous force, I would try to call attention to

how remarkably the whole of them, as methods of amelioration for the long considered impossibilities of this Edinburgh Equatorial, which Governments and governing Boards and Central Committees have so totally failed in through more than a dozen years,—flow from, or are bound up with, the one beautifully simple idea, of transferring the eye-piece from the upper end of the telescope, to near the opposite or outer end of the Declination Axis, by altering the angle only, of the small mirror, and not introducing any additional reflection.

It is this change which at once renders the Dome quite large enough for the telescope; which relieves the instrument of the immense amount of dead weight it was found unable to carry; which gives the observer a sheltered position to observe in, and his assistant plenty of room for working, either on one side or the other; while it also enables the modern science of spectroscopy to take up its position with power and dignity, in greater space than ever allowed to a star spectroscope before.

And where did this simple, yet all powerful and most suitable idea come from?

It was not with me, at any of the consultations over the instrument I assisted at many years ago. I have never heard it hinted at by any one else. Yet here it is now, because it very lately came to me. And came, I know not how, unless as a gracious gift from above, and at a moment of dire extremity, from the Giver of all Good.

Wherefore, if in this very advanced Christian age of the world, I were to hesitate for a moment, between misleadingly allowing the public to give the credit to me; or, on the other hand, attributing it myself frankly and thankfully to God, to whom it is alone due, I should deserve, like another person, well known by name, “to be eaten of worms and to give up the ghost”; instead of having been thus graciously preserved, through more than one generation of University Professors, up to the present moment;\* for further work, may it be, in elucidating the glory of the sidereal creations of the Divine Architect of all things.

#### APPENDIX I.

Memoranda of smaller and local practical matters, considered long ago as necessary to be attended to whenever a practical beginning

\* October 1887.



shall be made of what is described in the previous pages, and by whomsoever the work may be carried out.

(1) The present very thin flooring of the Dome will have to be propped up, substantially though temporarily only, before any heavy repairs begin.

(2) The large timber blocking of Declination axis to be exchanged for a small, compact, iron apparatus, or say merely a steel rod, passing by two small holes through a thick part of the Polar axis, but leaving its ends standing out sufficiently to butt against either one, or other side of the great cradle-frame of said axis; thereby serving the full purpose of the timber blocking, but without occupying one-hundredth part so much space, or fixing the Dome, or interfering with movements of workmen about the instrument.

(3) Revolving Telescope head, with all its weights and attachments to be removed; Telescope tube to be raised 5 inches in its collars (from great mirror end, as assumed below, to eye-end above); great mirror to be restored, and residual balance at end of Declination axis (after being shortened 14 inches) to be practically ascertained as speedily as possible; all superfluous weights being carried out of the Dome, noting how much.

(4) The shutter of the Dome to be altered from present plan of pivoting in the Zenith, to Messrs Cook's new plan of pivoting on the opposite side of base. The permanent opening of Dome being then taken right up to and 4 feet past the Zenith, with a breadth of nowhere less than 30 inches. A revolving ventilator of much larger diameter than the present one being then attached to the Zenith of the shutter, and three bull's-eyes illuminators introduced beneath Zenith and horizon. A separate fence of sheet steel about 3 feet high being made to slide horizontally across lower part of Dome's opening, to keep out the violence of the wind when not observing at very low altitudes; and the late Mr Grubb's advice of lining the iron dome, with non-heat-conducting wood, to be no longer delayed.

(5) All the air-passages in and around the pier in both Dome-room and under-Dome-room should be caulked with elastic tow, for otherwise both these rooms become a chimney of draughts to all the rest of the Observatory below their level; and when shutters are opened there, the draughts up into the Dome are very severe.

(6) The hot-water pipe system, heated from the Computing-room gas-stove, should be exchanged from the under-Dome-room, into the Laboratory, and even have its chief development there; with revolving cowls in place of simple ones on the roof. Incandescent electric lighting by gas-engine and dynamo to be introduced at all the instruments, because capable of giving light, without oxidizing gases, and without sensible and most pernicious heating.

(7) The front projection and elevating part of the so-called Observing chair should be at once removed, together with whatever else may prevent the mere back and wheels of the chair traversing freely both round and past, or from one side to the other of the Declination-axis end, shortened as above, preliminary to the said parts being converted into the new observer's travelling hut.

(8) All axles, bearings, &c., of both Dome, Equatorial, and Clock movement should, without any further loss of time, be taken out of their sockets, well cleaned and re-lubricated; or mischief may take place amongst them.

(9) Both the original proposition of a cylinder chronograph, as well as that for a duplicate speculum for the telescope, should be realised.

(10) An engineering opinion to be obtained as to the residual strength of the neck of the Declination axis, considering the holes cut into it by its maker, but not required on the Astronomer's herein proposed method of working.

(11) The toothed spur wheels of Lift to be replaced by endless-screw wheels, as being safer from accidental "stripping of teeth" under heavy loads.

(12) All the recommendations of the Government Committee of 1879, except such as the Astronomer may agree to dispense with, should be fully and faithfully carried out, before the new works treated of in the first part of this paper are commenced upon.

(13) And if by that time Government may have decided to rebuild the Royal Observatory, Edinburgh, on a better site, in a more modern manner and supply it with new instruments, as recommended by their Commission in 1876, taking however the Equatorial with them to a larger Dome,—then all the said new works should still be carried on there, omitting only the shortening of the Declination Axis, which will be better to be kept of its present full

length; eye-pieces, and slow motion handles being equally extended.

## APPENDIX II.

### THE FINANCIAL REQUIREMENTS AND DIFFICULTY.

Before the Astronomer consented to join in the Board of Visitors' project, about 1870, of applying to Government for a large Equatorial, he pointed out that such an instrument, even if once set up complete, would require further expenditure year after year to keep it fully efficient. And that the working with it would be so peculiarly onerous and responsible, that the salaries of the officers of the Royal Observatory, Edinburgh, already acknowledged to be at, or below, starvation point, should be raised more nearly to the level of those of other Observatories, or of any ordinary Government offices.

He was told in answer that all *that* was most certainly right, and would be brought about; while the Board of Visitors—whom Government had appointed years before expressly to advise them on such matters, and how to keep up the Observatory thereby in all future time as “a proper Royal Observatory,”—did most honourably proceed to frame a scheme of modest improvement not only to the Observers' salaries, but to the available income of the Observatory, to be expended by the Astronomer in instrumental repairs, experiments and improvements at his discretion.

Under these promising circumstances the Astronomer joined the application of the Board for the large Equatorial. That instrument was accordingly allowed by Government in 1871, was in part set up, under the authority of the Office of Works in London in 1872; and in the following year, when the erection was found very incomplete, the scheme of the Board of Visitors for increasing the salaries and available income of the Observatory to a point sufficient to finish, maintain, and work the instrument—for a long time not unfavourably entertained by Government,—was suddenly and finally disallowed.

The Board of Visitors indeed continued to apply to Government, as represented by the Home Office, until in 1876 the then Home Secretary, Mr. since Sir Richard, now Lord Cross, adopted the following expedient for escaping from the terms of agreement under

which the Astronomical Institute of Edinburgh had parted with their Observatory to Government nearly thirty years previously. That is, declining to listen to the long time accredited Board of Visitors, he appointed autocratically a Committee of his own to come down from London, and examine and report on the case. That Committee accordingly arrived in July of 1876, examined at the Observatory, sat and discussed in Queen Street, and then reported for a series of financial improvements of a similar, though altered character to those of the Board of Visitors, because including a rebuilding of the Observatory in a modern manner and on a new site.

But the Home Secretary thereupon declined to listen to his own Committee, and neglected all their recommendations, as well as those of the older Board of Visitors.

The venerable Mr Duncan M'Laren, then Senior M.P. for Edinburgh, moved thereupon in Parliament to have the Committee's Report publicly printed, which was done in 1877. Still however nothing came of it until 1879, when on account of further representations by the same watchful guardian of Scottish interests, the Home Secretary found it expedient to send another of his Committees to examine and report again. Confining itself however this time to the Equatorial, and without admitting the Astronomer to their Council, this Committee advised certain improvements, obtained a grant for executing them, and handed it over to the Office of Works, where it is believed either to remain still, or to have lapsed to the Treasury after doing little or nothing at the instrument.

This result however is perhaps not very much to be regretted, because the sum was not only absurdly insufficient to go through with all that was, and still more is, required for efficiency in the mere inorganic instrument,—but the previously admitted starvation of the Observatory in all its offices and its various means of doing good after its kind, was left absolutely untouched, and prevails to a degree of intensity, that were it on a larger scale, or nearer London, might, in its crying injustice, excite severe public animadversion, with questions as to the propriety of Home Rule being the only way to obtain justice for Scotland.

To compare the case in round numbers with another Royal Observatory nearer London headquarters, viz., that at Greenwich,



the following contrast comes out, in so far as I may have the correct figures:—

(1) Expended on Greenwich Equatorial and Dome between 1856 and 1879 under the direction of the Astronomer-Royal, it is believed . . .	£14,000	(1') Expended on Edinburgh Equatorial and Dome, under the control of the Office of Works, it is believed . . .	£3000
(2) Annual budget of Royal Observatory, Greenwich, . . . . .	7000	(2') Annual budget of Royal Observatory, Edinburgh, . . . . .	1000
(3) Salary of Chief Assistant at Royal Observatory, Greenwich, . . . . .	600	(3') Salary of Chief Assistant at Royal Observatory, Edinburgh, . . . . .	200
(4) New Objective and Dome for Greenwich Equatorial, 1885-86, perhaps . . .	5000	(4') New Objective and Dome for Edinburgh Equatorial, certainly . . . . .	0

But as every one, including myself most heartily, allows that the Greenwich Observatory is one of the best managed, most economical and most efficient Observatories and Government establishments in the world,—the result of the comparison is simply to show that the Edinburgh Observatory is most seriously underpaid.

Or to compare in detail the requisite remuneration of one worker in the Edinburgh Observatory, with what is allowed to be the correct thing in any ordinary Government Civil Office, please to compare the two following paragraphs which were printed in the public newspapers of this country *on the same day*.

(1) *Scotsman* Newspaper, December 6, 1880.

“An open competition is to be held in January 1881 simultaneously in London, Edinburgh, and Dublin for the post of Second Assistant Astronomer at the Royal Observatory at Edinburgh. Salary £100 *per ann.*” (Tempered however in fact with an extraneous and temporary addition rising by £10 *per ann.* to £50 *per ann.*, so long as certain extra work is carried on.)

(2) *Daily Telegraph* Newspaper, December 6, 1880.

“An open competition will shortly be held for two Junior Clerkships in the Colonial Office, with salaries commencing at £250 *per ann.* and rising to £600. Five of the Junior Clerks have additional emoluments. The higher clerkships, with salaries from £700 to £1000, are filled by promotion from the junior class.”

And again on Thursday, Feb. 12, 1885, the country was informed by the *Edinburgh Evening Express*, that a reorganisation of the Correspondence Department of the India Office had just taken place, leaving it thus,—

6 Secretaries at £1200 *per ann.* each.

7 Assistant Secretaries, £800 to £1000 each.

11 Senior Clerks, commencing with £600, and rise to £800.

12 Junior Clerks, commence with £200, and rise by £20 *per ann.* to £600.

Is not this a contrast of most severe kind to occur under the same Government? Especially when one learns further that the Colonial Office Clerks have only day work, and, as may be quite right, in very comfortable, well-warmed rooms of easily accessible buildings kept up at Government expense. While the Edinburgh Observatory Assistants have night, as well as day, work in an inclement little building perched on a hill-top more exposed to storms of wind, rain and snow, and more difficult to get at, or even to leave safely, in the dark than any other Astronomical Observatory or Government Office in any city of the land.

Or compare the £100 salary, possibly rising after a time and for a time only, to £150 *per ann.*, with the £1400 *per ann.* of a Clerk in the Treasury, recently defended in the public papers and insisted on as being only just payment for the hard work there, by a Liberal Prime Minister.

Now these matters, though apparently in my case tinged with personal feelings and sufferings too, yet cannot always, and may not much longer, appertain to me ; while they are otherwise and necessarily so intimately connected with the subject of the Edinburgh Equatorial,—if it is ever to be successfully worked and to prove an eventual honour to the country—that they cannot but be entered in any project for the scientific finishing and physical using of the instrument—the largest of its kind that has ever been seen in Scotland. And then it is also to be remembered that these matters were stipulated for, and promised to the Astronomer before the instrument was begun,—see the printed Reports of the Astronomer approved by the Board of Visitors—and that a Board, appointed nearly a generation before Sir Richard Cross, having entered the Home Office in London, obtained thereby supreme power over the Royal

Observatory, Edinburgh; ignored arrangements supposed to have been made for all perpetuity, and rendered impossible, even to the Central Office of Works in London, the completion of this hitherto unfortunate Equatorial tele-spectroscope. A grand beginning, however, of a first class instrument, it must be allowed; and still safely preserved under a sound wind-and-water-tight Dome for any eventualities which the future may be charged to bring along with it.

**5. On Cauchy's and Green's Doctrine of Extraneous Force to explain dynamically Fresnel's Kinematics of Double Refraction. By Sir William Thomson.**

1. Green's dynamics of polarisation by reflection, and Stokes's dynamics of the diffraction of polarised light, and Stokes's and Rayleigh's dynamics of the blue sky, all agree in, as seems to me, irrefragably demonstrating Fresnel's original conclusion, that in plane polarised light the line of vibration is perpendicular to the plane of polarisation; the "plane of polarisation" being defined as the plane through the ray and perpendicular to the reflecting surface, when light is polarised by reflection.

2. Now when polarised light is transmitted through a crystal, and when rays in any one of the principal planes are examined, it is found that—

(1) A ray with its plane of polarisation in the principal plane travels with the same speed, whatever be its direction (whence it is called the "ordinary ray" for that principal plane); and (2) a ray whose plane of polarisation is perpendicular to the principal plane, and which is called "the extraordinary ray" of that plane, is transmitted with velocity differing for different directions, and having its maximum and minimum values in two mutually perpendicular directions of the ray.

3. Hence, and by § 1, the velocities of all rays having their vibrations *perpendicular* to one principal plane are the same; and the velocities of rays in a principal plane which have their directions of vibration *in* the same principal plane, differ according to the direction of the ray, and have maximum and minimum values for directions of the ray at right angles to one another. But in the

laminar shearing or distortion motion of which the wave-motion of the light consists, the "plane of the shear"\* (or "plane of the distortion," as it is sometimes called), is the plane through the direction of the ray and the direction of vibration; and therefore it would be the *ordinary* ray that would have its line of vibration in the principal plane, if the ether's difference of quality in different directions were merely the aeolotropy of an unstrained elastic solid.† Hence ether in a crystal must have something essentially different from mere intrinsic aeolotropy; something that can give different velocities of propagation to two rays, of one of which the line of vibration and line of propagation coincide respectively with the line of propagation and line of vibration of the other.

4. The difficulty of imagining what this "something" could possibly be, and the utter failure of dynamics to account for double refraction without it, have been generally felt to be the greatest imperfection of optical theory.

It is true that ever since 1839 a suggested explanation has been before the world; given independently by Cauchy and by Green, in what Stokes has called their "Second Theories of Double Refraction," presented on the same day, the 20th of May of that year, to the French Academy of Sciences and the Cambridge Philosophical Society. Stokes, in his Report on Double Refraction,‡ has given a perfectly clear account of this explanation. It has been but little noticed otherwise, and somehow it has not been found generally acceptable; perhaps because of a certain appearance of artificiality and arbitrariness of assumption, which might be supposed to discredit it. But whatever may have been the reason or reasons which have caused it to be neglected as it has been, and though it is undoubtedly faulty, both as given by Cauchy and by Green, it contains what seems to me, in all probability, the true principle of the explanation, and which is, that the ether in a doubly refracting crystal is an elastic solid, unequally pressed or

\* *Thomson and Tail's Natural Philosophy*, § 171; (or *Elements*, § 150).

† The elementary dynamics of elastic solids show that on this supposition there might be maximum and minimum velocities of propagation for rays in directions at  $45^\circ$  to one another, but that the velocities *must essentially be equal for every two directions at  $90^\circ$  to one another* in the principal plane, when the line of vibration is in this plane.

‡ *British Association Report*, 1862.



unequally pulled in different directions by the unmoved ponderable matter.

5. Cauchy's work on the wave-theory of light is complicated throughout, and to some degree vitiated, by admission of the Navier-Poisson false doctrine\* that compressibility is calculable theoretically from rigidity; a doctrine which Green sets aside, rightly and conveniently, by simply assuming incompressibility. In other respects Cauchy's and Green's "Second Theories of Double Refraction," as Stokes calls them, are almost identical. Each supposes ether in the crystal to be an intrinsically aeolotropic elastic solid, having its aeolotropy modified in virtue of internal pressure or pull, equal or unequal in different directions, produced by and balanced by extraneous force. Each is faulty in leaving intrinsic rigidity-modulus (coefficients) unaffected by the equilibrium-pressure; and in introducing three fresh terms, with coefficients (A, B, C in Green's notation) to represent the whole effect of the equilibrium pressure. This gives for the case of an intrinsically isotropic solid, augmentation of virtual rigidity, and therefore of wave-velocity, by equal pull † in all directions, and diminution by equal positive pressure in all directions; which is obviously wrong. Thus definitively, pull in all directions outwards perpendicular to the bounding surface, equal per unit of area to three times the intrinsic rigidity-modulus, would give quadrupled virtual rigidity, and therefore doubled wave-velocity! Positive normal pressure inwards equal to the intrinsic rigidity-modulus would annul the rigidity and the wave-velocity; that is to say, would make a fluid of the solid. And on the other hand, negative pressure, or outward pull, on an incompressible liquid, would give it virtual rigidity, and render it capable of transmitting laminar waves! It is obvious that abstract dynamics can show, for pressure or pull equal in all directions, no effect on any physical property of an incompressible solid or fluid.

\* See Stokes, "On the Friction of Fluids in Motion and on the Equilibrium and Motion of Elastic Solids," *Camb. Phil. Trans.*, 1845; §§ 19, 20, reprinted in Stokes's *Mathematical and Physical Papers*, vol. i. p. 123; or Thomson and Tait's *Natural Philosophy*, §§ 684, 685; or *Elements*, §§ 655, 656.

† So little has been done towards interpreting the formulas of either writer that it has not been hitherto noticed that positive values of Cauchy's G, H, I, or of Green's A, B, C, signify pulls, and negative values signify pressures.

6. Again, pull or pressure *unequal in different directions*, on an isotropic incompressible solid, would, according to Green's formula (A) in p. 303 of his collected Mathematical Papers, cause the velocity of a laminar wave to depend simply on the wave-front, and to have maximum, minimax, and minimum velocities for wave-fronts perpendicular respectively to the directions of maximum pull, minimax pull, and minimum pull; and would make the wave-surface a simple ellipsoid! This, which would be precisely the case of foam stretched unequally in different directions, seemed to me a very interesting and important result, until (as shown in § 19 below) I found it to be not true.

7. To understand fully the stress-theory of double refraction, we may help ourselves effectively by working out directly and thoroughly (as is obviously to be done quite easily by abstract dynamics) the problem of § 6, as follows:—Suppose the solid isotropic, when unstrained, to become strained by pressure so applied to its boundary as to produce, throughout the interior, homogeneous strain according to the following specification:—

The coordinates of any point M of the mass which were  $\xi, \eta, \zeta$  when there was no strain, become in the strained solid,

$$\xi \sqrt{a}, \eta \sqrt{\beta}, \zeta \sqrt{\gamma} . . . . . (1);$$

$\sqrt{a}, \sqrt{\beta}, \sqrt{\gamma}$ , or the "Principal Elongations,"\* being the same whatever point M of the solid we choose. Because of incompressibility we have

$$a\beta\gamma = 1 . . . . . (2).$$

For brevity we shall designate as  $(\alpha, \beta, \gamma)$  the strained condition thus defined.

8. As a purely kinematic preliminary, let it be required to find the principal strain-ratios when the solid, already strained according to (1) (2), is further strained by a uniform shear,  $\sigma$ , specified as follows in terms of  $x, y, z$ , the coordinates of still the same particle, M, of the solid, and other notation as explained below:—

\* See chap. iv. of "Mathematical System of Elasticity" (W. Thomson), *Trans. R. S. Lond.*, 1856, reprinted in vol. iii. of *Mathematical and Physical Papers*, now on the point of being published; or *Thomson and Tail's Natural Philosophy*, §§ 160, 164; or *Elements*, §§ 141, 158.

$$\left. \begin{aligned} x &= \xi \sqrt{\alpha} + \sigma p l \\ y &= \eta \sqrt{\beta} + \sigma p m \\ z &= \zeta \sqrt{\gamma} + \sigma p n \end{aligned} \right\} \dots \dots \dots (3),$$

where  $p = OP = \lambda \xi \sqrt{\alpha} + \mu \eta \sqrt{\beta} + \nu \zeta \sqrt{\gamma} \dots \dots \dots (4),$

with  $l^2 + m^2 + n^2 = 1; \lambda^2 + \mu^2 + \nu^2 = 1 \dots \dots \dots (5),$

and  $l\lambda + m\mu + n\nu = 0 \dots \dots \dots (6);$

$\lambda, \mu, \nu$  denoting the direction-cosines of OP, the normal to the shearing planes; and  $l, m, n$  the direction-cosines of shearing displacement. The principal axes of the resultant strains are the directions of OM in which it is maximum or minimum, subject to the condition

$$\xi^2 + \eta^2 + \zeta^2 = 1 \dots \dots \dots (7);$$

and its maximum, minimax, and minimum values are the three required strain-ratios. Now we have

$$OM^2 = x^2 + y^2 + z^2 = \xi^2 \alpha + \eta^2 \beta + \zeta^2 \gamma + 2\sigma(l\xi \sqrt{\alpha} + m\eta \sqrt{\beta} + n\zeta \sqrt{\gamma})p + \sigma^2 p^2 \dots \dots (8):$$

and to make this maximum or minimum subject to (7), we have

$$\frac{d(\frac{1}{2}OM^2)}{d\xi} = \rho\xi; \quad \frac{d(\frac{1}{2}OM^2)}{d\eta} = \rho\eta; \quad \frac{d(\frac{1}{2}OM^2)}{d\zeta} = \rho\zeta \dots \dots (9);$$

where in virtue of (7), and because  $OM^2$  is a homogeneous quadratic function of  $\xi, \eta, \zeta$ ,

$$\rho = OM^2 \dots \dots \dots (10).$$

The determinantal cubic, being

$$(\mathcal{A} - \rho)(\mathcal{B} - \rho)(\mathcal{C} - \rho) - a^2(\mathcal{A} - \rho) - b^2(\mathcal{B} - \rho) - c^2(\mathcal{C} - \rho) + 2abc = 0,$$

where

$$\mathcal{A} = \alpha(1 + 2\sigma l\lambda + \sigma^2 \lambda^2); \quad \mathcal{B} = \beta(1 + 2\sigma m\mu + \sigma^2 \mu^2); \quad \mathcal{C} = \gamma(1 + 2\sigma n\nu + \sigma^2 \nu^2) \dots \dots (11),$$

and

$$a = \sqrt{(\beta\gamma)[\sigma(m\nu + n\mu) + \sigma^2 \mu\nu]}; \quad b = \sqrt{(\gamma\alpha)[\sigma(n\lambda + l\nu) + \sigma^2 \nu\lambda]}; \quad c = \sqrt{(\alpha\beta)[\sigma(l\mu + m\lambda) + \sigma^2 \lambda\mu]}. \dots (12),$$

gives three real positive values for  $\rho$ , the square roots of which are the required principal strain-ratios.

9. Entering now on the dynamics of our subject, remark that the isotropy (§ 7) implies that the work required of the extraneous pressure, to change the solid from its unstrained condition (1, 1, 1) to the strain ( $\alpha, \beta, \gamma$ ), is independent of the direction of the normal axes of the strain, and depends solely on the magnitudes of  $\alpha, \beta, \gamma$ .

Hence if  $E$  denotes its magnitude per unit of volume; or the potential energy of unit volume in the condition  $(\alpha, \beta, \gamma)$  reckoned from zero in the condition  $(1, 1, 1)$ ; we have

$$E = \psi(\alpha, \beta, \gamma) \dots \dots \dots (13),$$

where  $\psi$  denotes a function of which the magnitude is unaltered when the values of  $\alpha, \beta, \gamma$  are interchanged. Consider a portion of the solid, which, in the unstrained condition, is a cube of unit side, and which in the strained condition  $(\alpha, \beta, \gamma)$ , is a rectangular parallelepiped  $\sqrt{\alpha} \cdot \sqrt{\beta} \cdot \sqrt{\gamma}$ . In virtue of isotropy and symmetry, we see that the pull or pressure on each of the six faces of this figure, required to keep the substance in the condition  $(\alpha, \beta, \gamma)$ , is normal to the face. Let the amounts of these forces per unit area, on the three pairs of faces respectively, be  $A, B, C$ , each reckoned as positive or negative according as the force is positive *pull*, or positive pressure. We shall take

$$A + B + C = 0 \dots \dots \dots (14);$$

because normal pull or pressure uniform in all directions produces no effect, the solid being incompressible. The work done on any infinitesimal change from the configuration  $(\alpha, \beta, \gamma)$ , is

$$\left. \begin{aligned} & A \sqrt{(\beta\gamma)} d(\sqrt{\alpha}) + B \sqrt{(\gamma\alpha)} d(\sqrt{\beta}) + C \sqrt{(\alpha\beta)} d(\sqrt{\gamma}), \\ \text{or (because } \alpha\beta\gamma = 1) \\ & \frac{A}{2\alpha} d\alpha + \frac{B}{2\beta} d\beta + \frac{C}{2\gamma} d\gamma \end{aligned} \right\} \dots \dots (15).$$

10. Let  $\delta\alpha, \delta\beta, \delta\gamma$  be any variations of  $\alpha, \beta, \gamma$  consistent with (2): so that we have

$$\left. \begin{aligned} & (a + \delta a)(\beta + \delta\beta)(\gamma + \delta\gamma) = 1 \\ \text{and} & \alpha\beta\gamma = 1 \end{aligned} \right\} \dots \dots \dots (16).$$

Now suppose  $\delta\alpha, \delta\beta, \delta\gamma$  to be so small that we may neglect their cubes and corresponding products, and all higher products. We have

$$\frac{\delta a}{a} + \frac{\delta\beta}{\beta} + \frac{\delta\gamma}{\gamma} + \alpha\delta\beta\delta\gamma + \beta\delta\gamma\delta\alpha + \gamma\delta\alpha\delta\beta = 0 \dots \dots (17),$$

whence

$$\left(\frac{\delta a}{a}\right)^2 = \left(\frac{\delta\beta}{\beta} + \frac{\delta\gamma}{\gamma}\right)^2;$$

whence, and by the symmetrical expressions



$$\left. \begin{aligned} 2\delta\beta\delta\gamma &= \frac{1}{a} \left( \frac{\delta a^2}{a^2} - \frac{\delta\beta^2}{\beta^2} - \frac{\delta\gamma^2}{\gamma^2} \right) \\ 2\delta\gamma\delta a &= \frac{1}{\beta} \left( \frac{\delta\beta^2}{\beta^2} - \frac{\delta\gamma^2}{\gamma^2} - \frac{\delta a^2}{a^2} \right) \\ 2\delta a\delta\beta &= \frac{1}{\gamma} \left( \frac{\delta\gamma^2}{\gamma^2} - \frac{\delta a^2}{a^2} - \frac{\delta\beta^2}{\beta^2} \right) \end{aligned} \right\} \dots \dots (18).$$

11. Now if  $E + \delta E$  denote the energy per unit bulk of the solid in the condition

$$(a + \delta a, \beta + \delta\beta, \gamma + \delta\gamma);$$

we have, by Taylor's theorem,

$$\delta E = H_1 + H_2 + H_3 + \&c.$$

where  $H_1, H_2, \&c.$  denote homogeneous functions of  $\delta a, \delta\beta, \delta\gamma$ , of the 1<sup>st</sup> degree, 2<sup>nd</sup> degree, &c. Hence omitting cubes, &c., and eliminating the products from  $H_2$ , and taking  $H_1$  from (15), we find

$$\delta E = \frac{1}{2} \left( \frac{A}{a} \delta a + \frac{B}{\beta} \delta\beta + \frac{C}{\gamma} \delta\gamma + G \frac{\delta a^2}{a^2} + H \frac{\delta\beta^2}{\beta^2} + I \frac{\delta\gamma^2}{\gamma^2} \right) \dots (19),$$

where  $G, H, I$  denote three coefficients depending on the nature of the function  $\psi$ , (13), which expresses the energy. Thus in (19), with (14) taken into account, we have just five coefficients independently disposable,  $A, B, G, H, I$ ; which is the right number because, in virtue of  $a\beta\gamma = 1$ ,  $E$  is a function of just two independent variables.

12. For the case of  $a = 1, \beta = 1, \gamma = 1$ , we have  $A = B = C = 0$ ; and  $G = H = I = G_1$ , suppose; which give

$$\delta E = \frac{1}{2} G_1 (\delta a^2 + \delta\beta^2 + \delta\gamma^2).$$

From this we see that  $2G_1$  is simply the rigidity modulus of the unstrained solid; because if we make  $\delta\gamma = 0$ , we have  $\delta a = -\delta\beta$  and the strain becomes an infinitesimal distortion in the plane ( $xy$ ) which may be regarded in two ways as a simple shear, of which the magnitude is  $\delta a^*$  (this being twice the elongation in one of the normal axes).

13. Going back to (10), (11), and (12), let  $\sigma$  be so small that  $\sigma^3$  and higher powers can be neglected. To this degree of approximation, we neglect  $abc$  in (10), and see that its three roots are respectively

$$\mathcal{A} = \frac{b^2}{\mathcal{C} - \mathcal{A}} - \frac{c^2}{\mathcal{B} - \mathcal{A}}; \quad \mathcal{B} = \frac{c^2}{\mathcal{A} - \mathcal{B}} - \frac{a^2}{\mathcal{C} - \mathcal{B}}; \quad \mathcal{C} = \frac{a^2}{\mathcal{B} - \mathcal{C}} - \frac{b^2}{\mathcal{A} - \mathcal{C}} \quad (20),$$

\* Thomson and Tait's *Natural Philosophy*, § 175; or *Elements*, § 154.

provided none of the differences constituting the denominators is infinitely small. The case of any of these differences infinitely small, or zero, does not, as we shall see in the conclusion, require special treatment, though special treatment would be needed to interpret for any such case each step of the process.

14. Substituting now for  $\mathcal{A}$ ,  $\mathcal{B}$ ,  $\mathcal{C}$ ,  $a$ ,  $b$ ,  $c$  in (20) their values by (11) and (12); neglecting  $\sigma^3$  and higher powers; and denoting by  $\delta a$ ,  $\delta \beta$ ,  $\delta \gamma$  the excesses of the three roots above  $a$ ,  $\beta$ ,  $\gamma$  respectively, we find

$$\left. \begin{aligned} \delta a &= a \left\{ 2\sigma l\lambda + \sigma^2 \left[ \lambda^2 - \frac{\gamma}{\gamma-a}(n\lambda + l\nu)^2 - \frac{\beta}{\beta-a}(l\mu + m\lambda)^2 \right] \right\} \\ \delta \beta &= \beta \left\{ 2\sigma m\mu + \sigma^2 \left[ \mu^2 - \frac{a}{a-\beta}(l\mu + m\lambda)^2 - \frac{\gamma}{\gamma-\beta}(m\nu + n\mu)^2 \right] \right\} \\ \delta \gamma &= \gamma \left\{ 2\sigma n\nu + \sigma^2 \left[ \nu^2 - \frac{\beta}{\beta-\gamma}(m\nu + n\mu)^2 - \frac{a}{a-\gamma}(n\lambda + l\nu)^2 \right] \right\} \end{aligned} \right\} \dots (21);$$

and using these in (19), we find

$$\delta E = \sigma(A\lambda + Bm\mu + Cn\nu) + \frac{1}{2}\sigma^2 \left\{ A\lambda^2 + B\mu^2 + C\nu^2 + L(m\nu + n\mu)^2 + M(n\lambda + l\nu)^2 + N(l\mu + m\lambda)^2 \right\} + 2\sigma^2(Gl^2\lambda^2 + Hm^2\mu^2 + In^2\nu^2) \dots (22);$$

$$\text{where } L = \frac{B\gamma - C\beta}{\beta - \gamma}; \quad M = \frac{Ca - A\gamma}{\gamma - a}; \quad N = \frac{A\beta - Ba}{a - \beta} \dots (23).$$

15. Now from (5) and (6), we find

$$(m\nu + n\mu)^2 = 1 - l^2 - \lambda^2 + 2(l^2\lambda^2 - m^2\mu^2 - n^2\nu^2) \dots (24);$$

which, with the symmetrical expressions, reduces (22) to

$$\delta E = \sigma(A\lambda + Bm\mu + Cn\nu) + \frac{1}{2}\sigma^2 \left\{ L + M + N + (A-L)\lambda^2 + (B-M)\mu^2 + (C-N)\nu^2 - L^2 - M^2 - N^2 + 2[(2G+L-M-N)l^2\lambda^2 + (2H+M-N-L)m^2\mu^2 + (2I+N-L-M)n^2\nu^2] \right\} \dots (25)$$

16. To interpret this result statically, imagine the solid to be given in the state of homogeneous strain ( $a$ ,  $\beta$ ,  $\gamma$ ) throughout, and let a finite plane plate of it, of thickness  $h$ , and of very large area  $Q$ , be displaced by a shearing motion according to the specification (3), (4), (5), (6) of § 8; the bounding planes of the plate being unmoved; and all the solid exterior to the plate being therefore undisturbed, except by the slight distortion round the edge of the plate produced by the displacement of its substance. The analytical expression of this is

$$\sigma = f(p) \dots (26),$$

where  $f$  denotes any function of OP such that

$$\int_0^h dp f(p) = 0 \quad \dots \dots \dots (27).$$

If we denote by  $W$  the work required to produce the supposed displacement, we have

$$W = Q \int_0^h dp \delta E + W \quad \dots \dots \dots (28),$$

$\delta E$  being given by (25), with everything constant except  $\sigma$  a function of  $OP$ ; and  $W$  denoting the work done on the solid outside the boundary of the plate. In this expression the first line of (25) disappears in virtue of (27); and we have

$$\frac{W - W}{Q} = \frac{1}{2} \{ L + M + N + (A - L)\lambda^2 + (B - M)\mu^2 + (C - N)\nu^2 - L^2 - M^2 - N^2 \\ + 2[(2G + L - M - N)\lambda^2 + (2H + M - N - L)\mu^2 + (2I + N - L - M)\nu^2] \int_0^h dp \sigma^2 \dots (29).$$

When every diameter of the plate is infinitely great in comparison with its thickness,  $W/Q$  is infinitely small; and the second member of (29) expresses the work per unit of area of the plate, required to produce the supposed shearing motion.

17. Solve now the problem of finding, subject to (5) and (6) of § 8, the values of  $l, m, n$  which make the factor  $\{ \}$  of the second member of (29), a maximum or minimum. This is only the problem of finding the two principal diameters of the ellipse in which the ellipsoid

$$[2(2G + L - M - N)\lambda^2 - L]x^2 + [2(2H + M - N - L)\mu^2 - M]y^2 + [2(2I + N - L - M)\nu^2 - N]z^2 = \text{const.} \dots (30)$$

is cut by the plane

$$\lambda x + \mu y + \nu z = 0 \quad \dots \dots \dots (31).$$

If the displacement is in either of the two directions ( $l, m, n$ ) thus determined, the force required to maintain it is in the direction of the displacement; and the magnitude of this force per unit bulk of the material of the plate at any point within it is easily proved to be

$$\left\{ M \right\} \frac{d\sigma}{dp} \quad \dots \dots \dots (32),$$

where  $\{M\}$  denotes the maximum or the minimum value of the bracketed factor of (29).

18. Passing now from equilibrium to motion, we see at once that (the density being taken as unity)

$$V^2 = \{M\} \quad \dots \dots \dots (33),$$

where  $V$  denotes the velocity of either of two simple waves, whose wave-front is perpendicular to  $(\lambda, \mu, \nu)$ . Consider the case of wave-front perpendicular to one of the three principal planes; ( $yz$ ) for instance: we have  $\lambda = 0$ ; and, to make  $\{ \}$  of (29) a maximum or a minimum, we see by symmetry that we must either have

$$\left. \begin{array}{l} \text{(vibration perpendicular to principal plane)} \quad l=1, m=0, n=0 \quad . \quad . \\ \text{or (vibration in principal plane)} \quad l=0, m=-\nu, n=\mu \quad . \quad . \end{array} \right\} (34).$$

Hence, for the two cases, we have respectively

$$\text{Vibration perpendicular to } yz \quad . \quad . \quad V^2 = M + N + (B - M)\mu^2 + (C - N)\nu^2 \quad . \quad . \quad (35);$$

$$\text{Vibration in } yz \quad . \quad . \quad . \quad V^2 = L + B\mu^2 + C\nu^2 + 4(H + I - L)\mu^2\nu^2 \quad . \quad . \quad (36).$$

19. According to Fresnel's theory (35) must be constant, and the last term of (36) must vanish. These and the corresponding conclusions relatively to the other two principal planes are satisfied if, and require that,

$$A - L = B - M = C - N \quad . \quad . \quad . \quad (37),$$

$$\text{and} \quad H + I = L; \quad I + G = M; \quad G + H = N \quad . \quad . \quad (38).$$

Transposing  $M$  and  $N$  in the last of equations (37), substituting for them their values by (23), and dividing each member by  $\beta\gamma$ , we find

$$\frac{A - C}{\beta\gamma - a\beta} = \frac{B - A}{\gamma a - \beta\gamma} \quad . \quad . \quad . \quad (39):$$

whence (sum of numerators divided by sum of denominators),

$$\frac{B - C}{\gamma a - a\beta} = \frac{C - A}{a\beta - \beta\gamma} = \frac{A - B}{\beta\gamma - \gamma a} \quad . \quad . \quad . \quad (40).$$

The first of these equations is equivalent to the first of (37); and thus we see that the two equations (37) are equivalent to one only; and (39) is a convenient form of this one. By it, as put symmetrically in (40), and by bringing (14) into account, we find, with  $k$  taken to denote a coefficient which may be any function of  $(a, \beta, \gamma)$ :

$$\left. \begin{array}{l} A = k(S - \beta\gamma); \quad B = k(S - \gamma a); \quad C = k(S - a\beta) \end{array} \right\} \quad . \quad . \quad (41):$$

where

$$S = \frac{1}{3}(\beta\gamma + \gamma a + a\beta)$$

and using this result in (23), we find

$$\left. \begin{array}{l} L = k[a(\beta + \gamma) - S]; \quad M = k[\beta(\gamma + a) - S]; \quad N = k[\gamma(a + \beta) - S] \\ \text{or} \quad L = k(2S - \beta\gamma); \quad M = k(2S - \gamma a); \quad N = k(2S - a\beta) \end{array} \right\} \quad . \quad . \quad (42);$$

By (2) we may put (41) and (42) into forms more convenient for some purposes as follows:—



$$A = k\left(S - \frac{1}{\alpha}\right); \quad B = k\left(S - \frac{1}{\beta}\right); \quad C = k\left(S - \frac{1}{\gamma}\right) \quad . \quad . \quad (43),$$

$$L = k\left(2S - \frac{1}{\alpha}\right); \quad M = k\left(2S - \frac{1}{\beta}\right); \quad N = k\left(2S - \frac{1}{\gamma}\right) \quad . \quad . \quad (44),$$

where

$$S = \frac{1}{3}\left(\frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma}\right) \quad . \quad . \quad . \quad (45).$$

Next, to find G, H, I; by (38), (44), and (45), we have

$$G + H + I = \frac{1}{2}(L + M + N) = \frac{3}{2}kS = \frac{1}{2}k\left(\frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma}\right) \quad . \quad . \quad . \quad (46),$$

whence by (38) and (44),

$$G = k\left(\frac{1}{\alpha} - \frac{1}{2}S\right); \quad H = k\left(\frac{1}{\beta} - \frac{1}{2}S\right); \quad I = k\left(\frac{1}{\gamma} - \frac{1}{2}S\right) \quad . \quad . \quad (47).$$

20. Using (43) and (47) in (19), we have

$$\delta E = \frac{1}{2}k \left\{ \begin{aligned} & -\frac{\delta\alpha}{\alpha^2} - \frac{\delta\beta}{\beta^2} - \frac{\delta\gamma}{\gamma^2} + S\left(\frac{\delta\alpha}{\alpha} + \frac{\delta\beta}{\beta} + \frac{\delta\gamma}{\gamma}\right) \\ & + \frac{\delta\alpha^2}{\alpha^3} + \frac{\delta\beta^2}{\beta^3} + \frac{\delta\gamma^2}{\gamma^3} - \frac{1}{2}S\left(\frac{\delta\alpha^2}{\alpha^2} + \frac{\delta\beta^2}{\beta^2} + \frac{\delta\gamma^2}{\gamma^2}\right) \end{aligned} \right\} \quad . \quad . \quad . \quad (48).$$

Now we have, by (2)  $\log(\alpha\beta\gamma) = 0$ . Hence taking the variation of this as far as terms of the second order,

$$\frac{\delta\alpha}{\alpha} + \frac{\delta\beta}{\beta} + \frac{\delta\gamma}{\gamma} - \frac{1}{2}\left(\frac{\delta\alpha^2}{\alpha^2} + \frac{\delta\beta^2}{\beta^2} + \frac{\delta\gamma^2}{\gamma^2}\right) = 0 \quad . \quad . \quad . \quad (49);$$

which reduces (48) to

$$\delta E = \frac{1}{2}k\left(-\frac{\delta\alpha}{\alpha^2} - \frac{\delta\beta}{\beta^2} - \frac{\delta\gamma}{\gamma^2} + \frac{\delta\alpha^2}{\alpha^3} + \frac{\delta\beta^2}{\beta^3} + \frac{\delta\gamma^2}{\gamma^3}\right) \quad . \quad . \quad (50).$$

Remembering that cubes and higher powers are to be neglected, we see that (50) is equivalent to

$$\delta E = \frac{1}{2}k\delta\left(\frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma}\right) \quad . \quad . \quad . \quad (51).$$

Hence if we take  $k$  constant, we have

$$E = \frac{1}{2}k\left(\frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma} - \right) \quad . \quad . \quad . \quad (52);$$

and it is clear that  $k$  must be stationary (that is to say  $\delta k = 0$ ) for any particular values of  $\alpha, \beta, \gamma$  for which (51) holds; and if (51) holds for all values,  $k$  must be constant for all values of  $\alpha, \beta, \gamma$ .

21. Going back to (29), taking  $Q$  great enough to allow  $W/Q$  to be neglected, and simplifying by (46), (43), and (44) we find

$$\frac{W}{Q} = k \left( \frac{l^2}{a} + \frac{m^2}{\beta} + \frac{n^2}{\gamma} \right) \int_0^h dp \sigma^2 \dots \dots \dots (53);$$

and the problem (§ 17) of determining  $l, m, n$ , subject to (5) and (6), to make  $l^2/a + m^2/\beta + n^2/\gamma$ , a maximum or minimum for given values of  $\lambda, \mu, \nu$ , yields the equation

$$\varpi \lambda - \varpi' l + \frac{l}{a} = 0; \quad \varpi \mu - \varpi' m + \frac{m}{\beta} = 0; \quad \varpi \nu - \varpi' n + \frac{n}{\gamma} = 0, \quad (54),$$

$\varpi, \varpi'$  denoting indeterminate multipliers; whence

$$\varpi' = \frac{l^2}{a} + \frac{m^2}{\beta} + \frac{n^2}{\gamma} \dots \dots \dots (55),$$

$$\varpi^2 = l^2 \left( \varpi' - \frac{1}{a} \right)^2 + m^2 \left( \varpi' - \frac{1}{\beta} \right)^2 + n^2 \left( \varpi' - \frac{1}{\gamma} \right)^2 \dots \dots (56),$$

$$\left. \begin{aligned} \varpi \lambda &= l \left( -\frac{1-l^2}{a} + \frac{m^2}{\beta} + \frac{n^2}{\gamma} \right) \\ \varpi \mu &= m \left( \frac{l^2}{a} - \frac{1-m^2}{\beta} + \frac{n^2}{\gamma} \right) \\ \varpi \nu &= n \left( \frac{l^2}{a} + \frac{m^2}{\beta} - \frac{1-n^2}{\gamma} \right) \end{aligned} \right\} \dots \dots \dots (57).$$

These formulas are not directly convenient for finding  $l, m, n$ , from  $\lambda, \mu, \nu$ , given (the ordinary formulas for doing so need not be written here); but they give  $\lambda, \mu, \nu$  explicitly in terms of  $l, m, n$ , supposed known; that is to say, they solve the problem of finding the wave-front of the simple laminar wave whose direction of vibration is  $(l, m, n)$ . The velocity is given by

$$v^2 = k \left( \frac{l^2}{a} + \frac{m^2}{\beta} + \frac{n^2}{\gamma} \right) \dots \dots \dots (58).$$

It is interesting to notice that this depends solely on the direction of the line of vibration; and that (except in special cases, of partial or complete isotropy) there is just one wave-front for any given line of vibration. These are precisely in every detail the conditions of Fresnel's Kinematics of Double Refraction.

22. Going back to (35) and (36), let us see if we can fit them to double refraction with line of vibration *in* the plane of polarisation. This would require (36) to be the ordinary ray, and therefore re-

quires the fulfilment of (38), as did the other supposition: but instead of (37) we now have [in order to make (36) constant]

$$A=B=C \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (59),$$

and therefore each, in virtue of (14), zero; and therefore by (43),

$$\alpha = \beta = \gamma = 1 :$$

so that we are driven to complete isotropy. Hence our present form (§ 7) of the stress theory of double refraction *cannot* be fitted to give line of vibration *in* the plane of polarisation. We have seen (§ 21) that it *does* give line of vibration *perpendicular to the plane of polarisation with exactly Fresnel's form* of wave-surface, when fitted for the purpose by the simple assumption that the potential energy of the strained solid is expressed by (52) with  $k$  constant! It is important to remark that  $k$  is the rigidity-modulus of the unstrained isotropic solid.

23. From (58) we see that the velocities of the waves corresponding to the three cases,  $l=1$ ,  $m=1$ ,  $n=1$ , respectively, are  $\sqrt{(k/a)}$ ,  $\sqrt{(k/\beta)}$ ,  $\sqrt{(k/\gamma)}$ . Hence the velocity of any wave whose vibrations are in the direction parallel to any one of the three principal elongations, multiplied by this elongation, is equal to the velocity of a wave in the unstrained isotropic solid.

## 6. Exhibition of Models.

The President exhibited Models of the Minimal Tetrakaidekahedron. His paper on the subject is printed in the *London, Edinburgh, and Dublin Philosophical Magazine*, vol. xxiv. 5th series, p. 503, December 1887.

7. **Researches on Micro-Organisms, including ideas of a new Method for their destruction in certain cases of Contagious Diseases. Part II.** By Dr A. B. Griffiths, F.R.S. (Edin.), F.C.S. (Lond. and Paris), *Principal and Lecturer on Chemistry and Biology, School of Science, Lincoln; Science Master in the Lincoln Grammar School, &c.*

In the *Proceedings of the Royal Society of Edinburgh*, vol. xiv. [No. 123], pp. 97-106, there is a paper of mine under the above title. I wish in the present memoir to communicate to your dis-

tinguished Society further details relative to these investigations. The principle of these researches is to find some germicidal agent capable of destroying the microbes of disease, which have been proved to reside in the blood, and are the causes (directly or indirectly) of certain contagious diseases. At the same time, an aqueous solution of such an agent, while destroying the microbes of disease, must have very little or no detrimental action upon the blood. Having found such a substance, the rationale is to inject (hypodermically) a solution of the microbe-destroyer directly into the blood. By so doing, the destruction of the pathogenic organisms *in situ* would be the result.

In my last memoir on this subject (*loc. cit.*) aqueous solutions of salicylic acid were shown to materially interfere with the life-histories of certain micro-organisms. In the present paper an account will be given of the action of various antiseptic and germicidal agents upon certain microbes and their spores, as well as a practical application of my method in a particular case of advanced phthisis.

#### I. ALKALOIDS PRODUCED BY LIVING MICROBES.

It appears, as pathological investigations progress, the real cause in many cases of contagious diseases (although not in all) is the formation of certain poisonous compounds (ptomaines or alkaloids) by living microbes; rather than the idea that the mere presence of these microbes in the blood or tissues causes such diseases.

It will be remembered that in 1885 Pouchet discovered the ptomain formed by the *Comma bacillus*; and being very soluble, is easily absorbed into the system. Hence the rapidity of death following the first symptoms of the disease.

Amongst very recent work on the subject of ptomaines, produced by pathogenic and other microbes, we have the following:—(a) Dr O. Bocklisch (*Berichte der deutschen chemischen Gesellschaft*, vol. xx. p. 1441) found that *Vibrio proteus* produced in contact with sterilised beef cadaverine ( $C_5H_{14}N_2$ ) which had been proved by Ladenburg (*Berichte der deutschen chemischen Gesellschaft*, vol. xix. p. 2585) to have all the chemical properties of pentamethylene-diamine ( $C_5H_{14}N_2$ ). This alkaloid or ptomaine of *Vibrio proteus* (Finkler's *bacillus*) is non-poisonous. Bocklisch went a step further, and found that when *Vibrio proteus* was allowed to live upon



sterilised beef along with putrefactive germs, besides cadaverine, a very poisonous base methylguanidine is the chief product of their life-histories. (b) Brieger (*Berichte*, vol. xix. p. 3119) has succeeded in isolating an alkaloid, which he calls tetamine ( $C_{13}H_{30}N_2O_4$ ) from pure cultivations of the bacillus, which causes traumatic tetanus in animals. (c) Although it has not been isolated, M. Pasteur believes that the virus of hydrophobia is a microbe, and that it produces an alkaloid. (d) Dr E. Alvarez (*Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences*, vol. cv. [No. 5], 1st August 1887) describes a microbe which he has proved to be the cause of the indigotic fermentation and the production of indigo-blue. This microbe is an encapsuled bacillus (fig. 1), similar

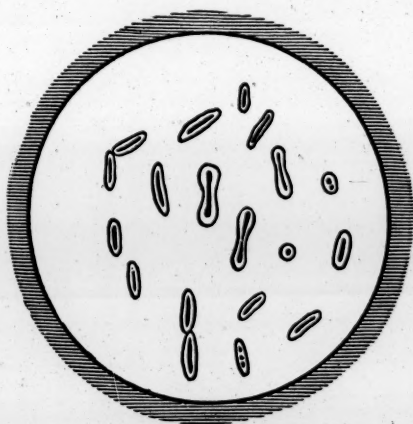


FIG.—1. Bacillus of Indigo Fermentation.

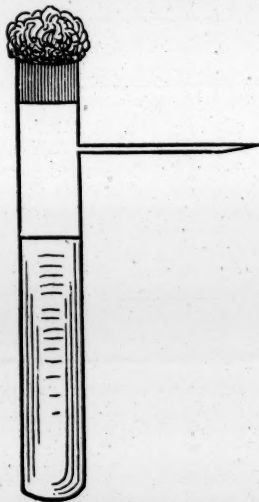


Fig. 2.

in appearance to the bacillus of Rhinoscleroma (Cornil and Alvarez). This bacillus of the indigo fermentation is shown to possess pathogenic properties, and occasions in animals a transient local inflammation, or death, with visceral congestion and fibrinous exudations. (e) It has been shown by Duclaux (in his work on *Ferments et Maladies*) that when the ptomaine produced by *Bacterium cholerae gallinarum* (which possesses narcotic properties) is separated, by filtration through a Chamberland filter, from its microbe, it does not produce fowl cholera, but causes a passing sleep, which does not generally end fatally.

I have alluded here in passing to recent work on the secretions or products formed during the life-histories of certain microbes,—and

it appears that the most important problems requiring the attention of the pathological worker are—(1) To isolate the microbes and their alkaloïds in a given contagious disease, and to study their chemical and pathological actions ; (2) To destroy the microbes (if they reside in the blood) *in situ* by hypodermic injections of some germicide.

## II. CELLULOSE THE PRODUCT FORMED BY CERTAIN MICRO-ORGANISMS.

In the *Journal of the Chemical Society* [Trans.], 1886, p. 432, Mr Adrian J. Brown, F.C.S., describes an acetic ferment, called by him *Bacterium xylinum*, which forms cellulose ; the substance of the membranous growth of the so-called "vinegar-plant," or the "Essighautchen" of Dr Zopf.

In my previous paper on this subject (*loc. cit.*) I alluded to the fact that Dr E. Freund had discovered that *Bacillus tuberculosis* forms cellulose. My own work on this micro-organism (to be described in this present memoir) entirely confirms Freund's discovery, and somewhat extends his observations. He found cellulose in the *organs* and *blood* of tuberculous persons, and I may add that cellulose is also to be found in the *sputa* of patients suffering from acute general phthisis. This was proved by the reactions used by Freund (see *Nature*, vol. xxxiv. p. 581) for the detection of cellulose in tuberculosis.

## III. ACTION OF CERTAIN ANTISEPTICS AND DISINFECTANTS UPON VARIOUS MICRO-ORGANISMS.

I have already shown that a solution of salicylic acid is a germicidal agent of a large number of micro-organisms ; and at this point I wish to detail several experiments undertaken to see the action of various reagents upon the life-histories of certain microbes.

### (a) *Sarcina lutea*.

Several Aitken's tubes (fig. 2) containing sterilised beef-broth (neutral) were taken and treated as follows :—

Tube No. I. was inoculated with the chromogenic saprophyte *Sarcina lutea* (from a pure cultivation in nutrient agar-agar), and

kept at a temperature of 40° C. They grew rapidly, and after four days formed a yellow pellicle upon the surface of the broth.

Tubes Nos. II. and III. contained sterilised beef-broth; and to the broth in each tube was added iodine (in the proportion of 1 millogramme of iodine to 100 c.c. of broth). The tubes were then inoculated with *Sarcina lutea* from the same source as Tube No. I. No growths made their appearances after the elapse of twenty-eight days, although the tubes were kept at the most favourable temperature for the development of this micro-organism. After the elapse of twenty-eight days, sterilised platinum needles were dipped into tubes No. II. and No. III., and the contents of four tubes containing sterilised nutrient agar-agar were inoculated from them. They remained in the incubator at 40° C. for twenty-one days, without any growths making their appearances in the tubes.

Other germicidal agents were tried (in a similar manner to the experiments just described) upon *Sarcina lutea*. Amongst these reagents the following proved fatal to the micro-organism:—

0·5 per cent. solution (sterilised beef broth) of potassium iodate.

3·0    "       "       "       "       "       salicylic acid.

0·4    "       "       "       "       "       sodium fluosilicate.

(b) *Micrococcus prodigiosus*.

I have already shown that salicylic acid is fatal to the growth and multiplication of this organism (see Part I. of this paper). Since the above experiments were performed upon this organism I have tried other experiments. In the preparation of sterilised nutrient agar-agar (according to the well-known methods) the above quantities of potassium iodate, salicylic acid, and sodium fluosilicate were added before filtration. After preparing a series of tubes containing sterilised nutrient agar-agar, with and also without the germicidal agent, they were all inoculated (the utmost care being observed) from pure cultivations of *Micrococcus prodigiosus* (fig. 3, A).

Tube No. I. was inoculated by means of a sterilised platinum wire from the potato cultivation of the micro-organism. After five days' growth at 34° C. in an incubator, the appearance was similar to the growth in fig. 3, B. (The colour was crimson.)

Tube No. II. contained, in addition to the nutrient agar-agar, 3 per cent. of salicylic acid.

Tube No. III. contained, in addition to the nourishing medium, 0.5 per cent. of potassium iodate.

Tube No. IV. contained, in addition to the sterilised agar-agar, 0.4 per cent. of sodium fluosilicate.

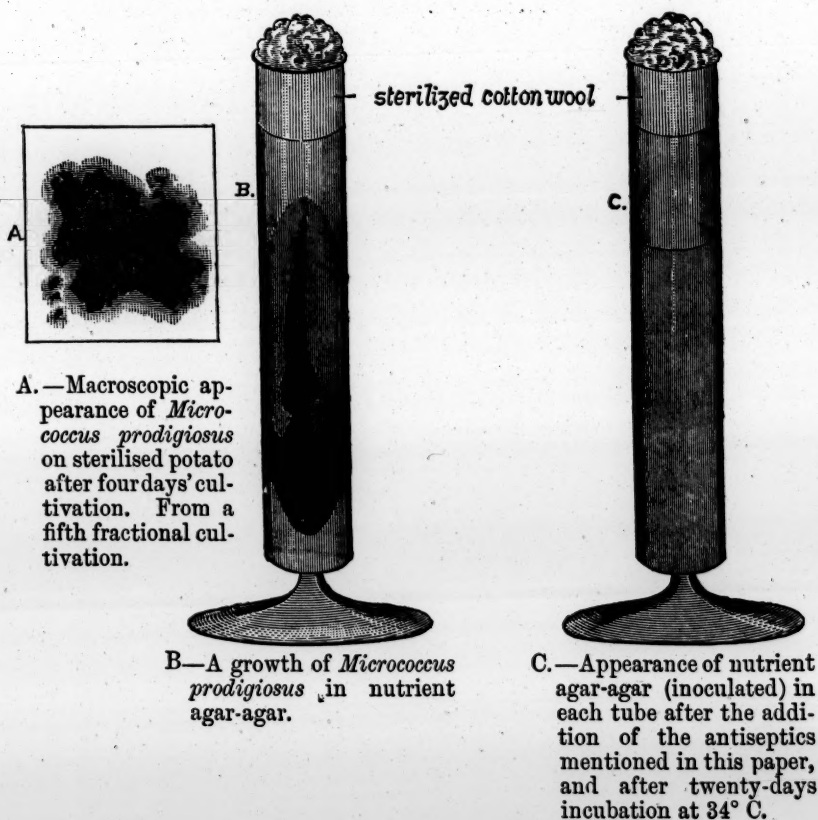


FIG. 3.—*Micrococcus prodigiosus*.

Tube No. V. contained 1 milligramme of iodine in 100 c.c. of the medium.

Tubes Nos. II., III., IV., and V. did *not* develop any growths after twenty-five days' incubation at a temperature of 34° C. After this period had elapsed, sterilised platinum needles were plunged into each tube, and were then transferred to four tubes containing sterilised nutrient agar-agar. No growths made their appearances in any of the tubes after the elapse of three weeks. All the above experiments were performed in duplicate with similar results.



(c) *Micrococcus tetragonus*.

This micrococcus (fig. 4) is found in the sputum of patients suffering with phthisis. According to the most reliable sources, *Micrococcus tetragonus* is only saprophytic in man, but pathogenic in animals. Mice inoculated with a small quantity die in a few days

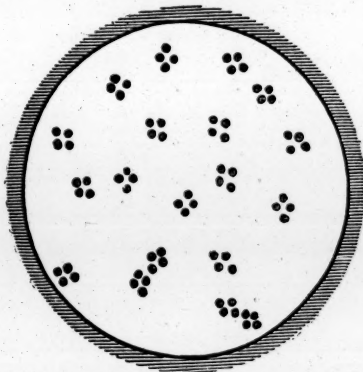


FIG. 4.—*Micrococcus tetragonus*, stained with gentian violet (much enlarged).

and the microbe afterwards is to be found in various organs of the body. This microbe grows tolerably well in nutrient agar-agar.\* I have experimented with *Micrococcus tetragonus* in an exactly similar manner to the experiments with *Micrococcus prodigiosus*, and obtained similar results. The agents used completely destroyed this micro-organism.

The sputum for this purpose was kindly sent to me on 19th July 1887, by Dr R. Wood, M.D., L.R.C.P. (Edin. & Lond.), &c., of Bromsgrove, Worcestershire, from one of his patients. The bottle sent to me was labelled :—"Thomas Smith (young man), expectorations of supposed phthisis at base of left lung. Sister died of it." I found in the sputum a considerable number of *Bacillus tuberculosis*, *Micrococcus tetragonus*, and a large quantity of Freund's cellulose.

(d) *Bacillus butyricus*.

It will be remembered that in my last memoir (*loc. cit.*) on this subject, I gave an account of having destroyed *Bacillus butyricus* by using the germicide salicylic acid in small quantities. This fact has recently been confirmed by M. Pierre Grosfils. M.

\* Agar-agar can be obtained from Christy & Co., 25 Lime Street, London, at 3s. per lb.

Grosfils communicated a paper to the Société d'Encouragement de Vervier; and it has recently been published in the *Moniteur Industriel*, describing a method for preserving butter from the action of *Bacillus butyricus* by the addition of 0·0002 per cent. of salicylic acid. Butter so treated will keep for an indefinite time, even in warm countries.

(e) *A New Micro-Organism.*

A new micro-organism, I have recently found upon putrefying onions kept in a warm, damp, and dark place. The cells are about 0·005 to 0·007 mm. long, and about 0·0025 mm. in width. This microbe is capable of forming the zooglœa state.

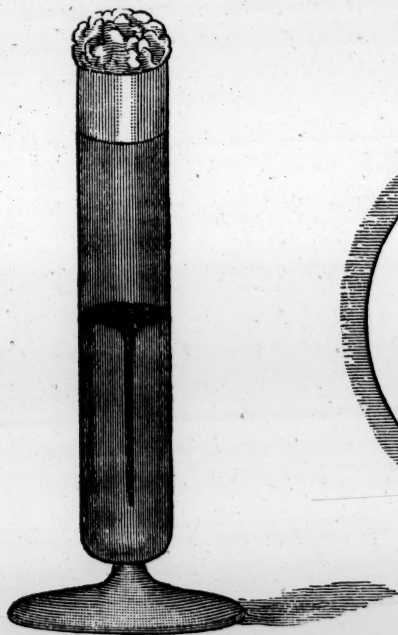


FIG. 5.—*Bacterium allium* (a new micro-organism) growing on nutrient agar-agar (after *Nature*, but not the colour).

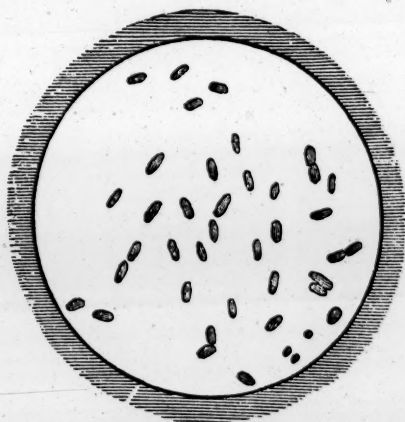


FIG. 6.—*Bacterium allium* (under high power), stained with gentian violet. The cells are colourless. The green colouring matter (which is insoluble in water, soluble in alcohol) resides in the interstitial substance.

They grow tolerably well in nutrient agar-agar, and produce a bright green pellicle upon the surface of the nourishing medium (fig. 5). This micro-organism, which causes putrefaction in onions, liberates small quantities of sulphuretted hydrogen gas. The sulphuretted hydrogen was proved by the black stain (PbS) produced upon paper impregnated with a solution of lead acetate; and also the yellow stain (CdS) produced by using cadmium paper ( $\text{CdCl}_2$ ). This sulphur gas is also produced to a small extent in the nutrient

agar-agar during an artificial cultivation of the microbe in that medium. The microbe stains best with gentian violet (fig. 6). I propose to call this microbe *Bacterium allium*, because it was discovered upon *Allium cepa*.

*Bacterium allium* is destroyed by the reagents described under the head of *Micrococcus prodigiosus*.

The colouring matter formed during the life-history of *Bacterium allium* is soluble in alcohol. Fig. 6a (III.) gives the absorption spectrum of the pigment in alcohol.

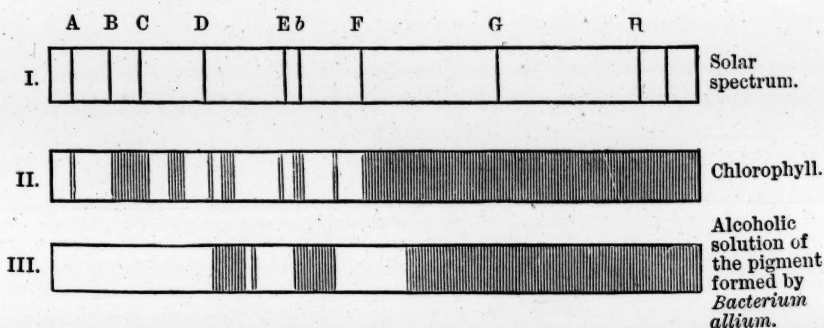


FIG. 6a.—Absorption Spectrum of an alcoholic solution of the green pigment formed during the life-history of *Bacterium allium*.

It will be noticed that there is an absorption band extending from the extreme violet to the greenish blue part of the spectrum. Also, an absorption band in the green and one in the yellow part of the spectrum. The end of the band in the yellow is exactly in the same position as the D Fraunhofer line in the solar spectrum. It will also be seen from fig. 6a that the spectrum produced by this pigment differs from chlorophyll, although both solutions were of the same intensity of colour, and nearly the same thickness, when placed in front of the slit of the spectroscop.

#### (f) *Various Micro-Organisms.*

The following micro-organisms were destroyed by the germicides already mentioned :—

- (1) *Micrococcus citreus conglomeratus*. (obtained from the dust of the air).
- (2) *Bacterium ureæ*.
- (3) *Bacterium indicum*.
- (4) *Micrococcus violaceus*.
- (5) *Sarcina aurantiaca*.

(g) *Penicillium glaucum*.

*Penicillium glaucum* grows well in flour-paste in a warm damp place. It is destroyed by salicylic acid, iodine, potassium iodate, and sodium fluosilicate; for no growths made their appearance in flour-paste (inoculated with the spores of this fungoid growth) after forty-six days' incubation.

IV. THE VITALITY OF *Bacillus tuberculosis* AND ITS SPORES.

In March of the present year (1887) I received from Mr John Snodgrass,\* jun., of Glasgow (who is suffering with acute general phthisis) typical specimens of sputum, which contained a large quantity of old discoloured blood, also lung fibre, débris of various kinds, and numbers of *Bacillus tuberculosis*. Fig. 8a is a drawing from a cover-glass preparation.

Small quantities of sputum were mixed with calcium sulphate and calcium carbonate, previously sterilised at a temperature of 135° C., and these mixtures were placed in sterilised tubes (fig. 7), and then hermetically sealed. Twelve of these dry tubes, each contained about 10 grammes of the mixture. Twelve dry sterilised tubes (fig. 8), not hermetically sealed, also contained about 10

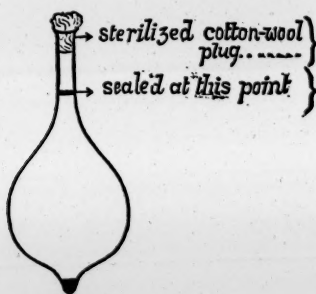


Fig. 7.

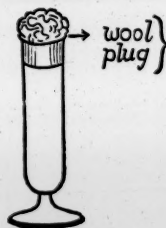


Fig. 8.

grammes of the mixture of sputum, calcium sulphate, and calcium carbonate (these mineral substances constituting the principal ingredients contained in the dust of the atmosphere). The twenty-four tubes were kept at a dry heat of 32° C., from one to six months. Two hermetically sealed tubes and two of the open tubes were opened after being exposed to a dry heat of 32° C. for one

\* The translator of Heine's "Religion and Philosophy in Germany," also "Wit, Wisdom, and Pathos, from the Prose of Heinrich Heine." (Trübner & Co.)



month; and four tubes, containing sterilised blood serum, were inoculated from the contents of the tubes. In the two inoculated from the open tubes, growths of *Bacillus tuberculosis* (proved by staining and microscopical appearance, &c.) made their appearances in sixteen days from the time of inoculation. Growths of *Bacillus tuberculosis* also made their appearance in the two tubes (after being inoculated from the contents of the sealed tubes) after nineteen days' incubation. Four more tubes were opened after being

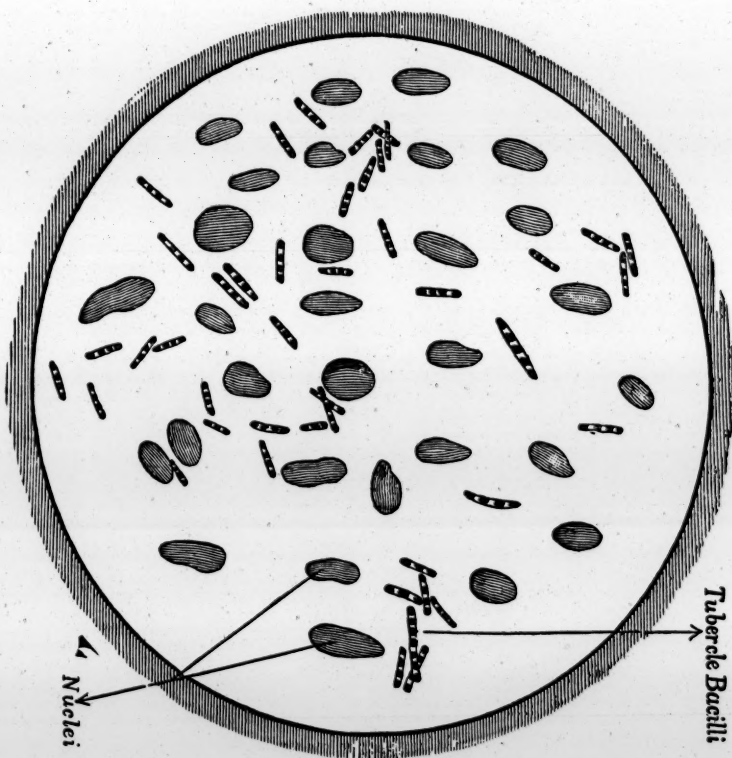


FIG. 8a.—*Bacillus tuberculosis* in Acute General Phthisis; from sputum of Mr John Snodgrass, jun. Stained by the Weigert-Ehrlich method.  $\times$  about 1400.

exposed for two months at the temperature already mentioned. Inoculations from two open tubes revealed the vitality of the *Bacillus tuberculosis* after twenty days' incubation; and inoculations from the two sealed tubes proved the vitality of the bacilli after the elapse of twenty-three days' incubation. The remaining tubes were examined in a similar manner after the elapse of three, four, five, and six months respectively.

After being exposed to the dry heat for three and four months, the vitality of this micro-organism and its spores was *not* destroyed. But, after being heated for five and six months the vitality of the microbe was completely destroyed; for no growths made their appearance in sterilised blood serum kept at a temperature between 37° and 39° C. for nearly two months.

From these experiments it will be seen that *Bacillus tuberculosis* is capable of being dried up in the dust of the atmosphere for several months without its vitality being impaired.

V. *Bacillus tuberculosis* DISSEMINATED BY FLIES, PAPER, &c.

It has been shown that farm animals may be inoculated through the bite of flies with *Bacillus anthracis* (the Bacteridia of Davaine); and Pasteur (*Bulletin de l'Académie de Médecine*, 1880) has shown that the casts of *Lumbricus terrestris* may contain the germs of splenic fever, at the same time possessing all their original virulence. Recently, MM. Spillman and Haushalter (*Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences*, vol. cv. [No. 7], 16th August 1887) have discovered that the common house fly in consumptive hospitals is very often seen upon the expectorations of the patients. Some of these flies were caught and placed under bell-glasses, and subsequently it was found that their excrements contained numbers of *Bacillus tuberculosis*.

Recently, I have cultivated (using every possible aseptic precaution) from the envelopes containing the letters from Mr Snodgrass (already mentioned), in sterilised solid blood serum growths which had all the macroscopical appearances of *Bacillus tuberculosis*. These pure cultivations gave serpent-like twistings in cover-glass impressions,—and under the higher powers of the microscope the characteristic form of *Bacillus tuberculosis* when stained by the Ehrlich and other methods.\*

From this investigation we draw the conclusions—(1) that the saliva of consumptive patients used in moistening an envelope may

\* Although I had been experimenting with *Bacillus tuberculosis* for some time, there were no chances of my cultivation plates and tubes becoming contaminated with *Bacillus tuberculosis* from sputum, &c., or with foreign microbes. They were inoculated from the envelopes in a room (with closed doors and windows) away from my laboratories; and further, I had changed my clothes and disinfected my hands.

contain the germs of phthisis; (2) that these germs are capable of travelling a distance of over 200 miles, and then growing, when a suitable medium and temperature ( $36^{\circ} - 39^{\circ} \text{ C.}$ ) are provided for them.

# VI. ELECTRICAL EXPERIMENTS ON THE *Bacillus tuberculosis* AND ITS SPORES.

The action of the electric current upon the vitality of various micro-organisms has been very little studied; therefore, perhaps, the following notes may be of interest.

The experiments were performed on pure cultivations of micro-organisms growing in fluid blood serum (slightly alkaline), and other media. (See fig. 9, representing the general arrangements).

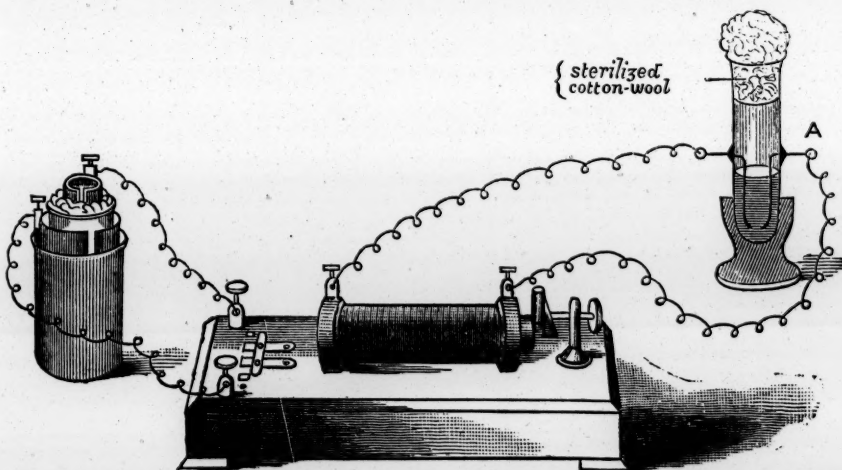


FIG. 9.—Electrical Experiments on the Vitality of *Bacillus tuberculosis* and its spores, &c. A—a tube containing growing bacilli in sterilised fluid blood serum slightly alkaline.

(1) *Bacterium lactis*, growing in previously sterilised milk, is killed by an E.M.F. of 2·26 volts.

(2) *Bacterium aceti*, growing in previously sterilised alcohol (7 per cent.), is killed by an E.M.F. of 3·24 volts.

(3) *Bacillus tuberculosis*, growing in previously sterilised fluid blood serum, is killed by an E.M.F. of 2·16 volts.

The temperature of the room was  $16^{\circ} \text{ C.}$  After allowing the current to pass for 10 minutes in each case, ten tubes containing sterilised fluid blood serum were inoculated from the “electrified”

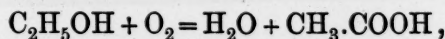
(if I may use that expression) tubercle-bacilli, and after being kept at a temperature of 38° C. for twenty-five days, *no* growths made their appearance in any of the tubes.

A similar number of tubes containing sterilised sweet milk were inoculated from the "electrified" *Bacterium lactis*, with *no* results after twenty-five days' incubation. Seven tubes containing the *purest* ethyl alcohol and ordinary filtered tap-water\* (the mixture contained 6 per cent. of alcohol) were inoculated with the "electrified" *Bacterium aceti*, with negative results.

So we have here positive evidence that these micro-organisms were destroyed by the electric current.

#### VII. IS *Bacterium aceti* THE REAL CAUSE OF THE ACETIC FERMENTATION ?

Although Pasteur maintained that *Bacterium aceti* was the cause of the acetic fermentation ; and Cohn (*Biol. d. Pflanzen*, vol. ii. p. 173) observed the micro-organism largely' in sour beers ; yet, *not* until the commencement of 1886, could any one say with certainty that this micro-organism was the real cause of the acetic fermentation. In that year, Mr Adrian J. Brown, F.C.S. (*Journal Chemical Society* [Trans.], 1886, p. 172), prepared *pure* cultivations of *Bacterium aceti*, and found that the well-known reaction—



is produced by the life-history of *Bacterium aceti* (*Mycoderma aceti*).

I can entirely endorse the correctness of Mr Brown's observations, for after obtaining pure cultivations of the micro-organism by a combination of the fractional and dilution methods (used by Brown), it was found that these cultivations, when used to inoculate sterilised ethyl alcohol (6 per cent.) gave acetic acid in abundance.

#### VIII. ANAL AND HYPODERMIC INJECTIONS OF AQUEOUS SOLUTIONS OF SALICYLIC ACID IN CASES OF CHOLERA.

It will be remembered that early in the present year (1887) the

\* Tap-water was used in preference to distilled water, on account of the mineral matter it contains—the micro-organisms requiring small quantities of mineral matter.



papers were full of accounts of "human beings dying in heaps" from cholera in the province of Cordova, in the Argentine Republic. Is there no cure for cholera? Or, in other words, is there no agent that will destroy Koch's *bacillus* in the human body? In passing, I may say, through reading the various newspaper abstracts of my memoir (*Proc. Roy. Soc. Edin.*, vol. xiv. pp. 97-106), read before the Society on January 31, 1887, Mr T. F. Agar (Consul-General for the Argentine Republic in Scotland) kindly wrote for a copy of my memoir. A written copy was forwarded to him, and he has presented it to his Government at Buenos Ayres. I am to have full details of any experiments performed on behalf of the Government of the Argentine Republic bearing on my injection method in cases of cholera.\*

It has been suggested some few years ago, that rum or cognac, containing 25 grammes of salicylic acid to the litre,† should be taken when cholera is epidemic.

If salicylic acid proves useful as a germicide, or even a preservative, from the severer attacks of Koch's *Bacillus komma*, would not anal and hypodermic injections of solutions of the acid be the best method of combating this disease? By these two kinds of injections, we should meet the growths of the microbe in the intestines, and also those that may have passed into the blood system by absorption.

Koch has remarked that acids in general are the greatest hindrance for the development of the cholera *bacillus*; and Dr Klein, F.R.S. (*Micro-Organisms and Disease*, p. 256), says—"Pathogenic organisms do not thrive in an acid medium." At any rate, whether the germicidal agent or medicament used be salicylic acid or one more powerful; I think that such a method as the one described would be the most rational, and evidently would possess a scientific basis, namely, *the destruction of microbes in situ*.

In the case of disinfecting a whole district against the cholera epidemic, the late Dr Wm. Budd, F.R.S., placed in the sewers of Bristol *ferrous sulphate*. Dr Budd says:—"In this way a chemical

\* Any information received from this source will be embodied in another paper communicated to the Royal Society of Edinburgh.

† "Three teaspoonfuls of the mixture to be taken between meals in coffee or tea."

bed was prepared for the poison, by whose action the population was ensured against harm from any specific germs that by accident or other cause might find their way into the drains or sewers of the town. The sulphate of iron in the drain, thus lying in waiting for the poison, may be likened to the wire gauze of the Davy lamp, always at hand to prevent the explosion of the fatal fire-damp.\*

I have shown (*Chemical News*, vol. xlix. p. 279; vol. liii. p. 255; vol. lv. p. 276; *Journal Chemical Society* [Trans.], 1886, p. 119; and *Chemiker-Zeitung*, No. 47) that ferrous sulphate destroys parasitic fungi; and it is probable that on a *large scale* (for sewers, &c.) it would form a cheap and powerful disinfectant against epidemic diseases in general.

#### IX. SOLUBLE ZYMASES AND THEIR MICROBES.

What have the soluble zymases (ferments) produced by various pathogenic microbes to do, in connection with contagious diseases? Are they the cause of the disease directly or indirectly? By their chemical disintegration, do they form the alkaloïds (ptomaines) found in disease? These problems require our earnest attention.

Dr Schiavuzzi of Pola (Istria in Austria) (*Rendiconti della R. Accademia dei Lincei*, December 1886) has confirmed Kleb's and Tommasi-Crudeli's† discovery of *Bacillus malarix*, and that it is the real cause (directly or indirectly) of malarial fever. Schiavuzzi also finds that in the blood of animals infected with the disease, the red corpuscles undergo similar alterations as Marchiafava and Celli (*Fortschr. d. Med.*, vol. iii.) have shown to be characteristic of malarial fever; and he considers these changes in the blood corpuscles to be caused by a "pathological" ferment of a different nature to *Bacillus malarix*. Most probably a soluble zymase secreted by the microbe itself. Professor Giglioli, in his recent work *Fermenti e Microbi*, describes the production of *soluble ferments* by micro-organisms.

If the soluble zymases produced by living pathogenic microbes

\* *The Cholera Microbe, and how to Meet it*, by Sir C. Cameron, LL.D., M.P., &c., p. 25.

† *Archiv für Experimental Pathol.*, 1879; and also Tommasi-Crudeli's memoir, "*Der Bacillus malarix* in Erdboden von Seliunte und Campobello," *Archiv für Exp. Pathol.*, 1880.

are the real cause of disease, the hypodermic injection method steps in, for many substances are known to interfere with the action of soluble ferments.\* The destruction of the microbes prevents the formation of soluble zymases or alkaloïds; and any given contagious disease (under these circumstances) would be at an end. Nature would then have a chance of restoring to their "normal standard the lowered vitality which enabled the microbes to get a footing."

#### X. SALICYLIC ACID, NATURAL AND ARTIFICIAL.

In the present paper it has been shown that salicylic acid is a good germicide. The *natural* acid prepared from oil of winter green (*Gaultheria procumbens*) is a far more powerful germicide than the "artificial" salicylic acid prepared from sodium phenate ( $C_6H_5NaO$ ). Hence, it appears from the above observations that the *natural* variety possesses properties which are not to be found in artificial salicylic acid. This fact supports Pasteur's idea (*Revue Scientifique*, January 5, 1884) that organic compounds prepared by *synthesis* are not altogether identical with the natural compounds. "Life" brings into play asymmetrical molecular forces, while in the mineral kingdom and also in our laboratories, *only* symmetrical molecular forces come into play. Pasteur's theory is summed up by M. Wyrouboff in these words:—

"Ces théories sont fondées sur une première hypothèse, qui suppose les phénomènes naturels soumis à deux sortes d'actions; les unes symétriques, les autres dissymétriques; les premières président à la minéralité et aux synthèses de nos laboratoires, les secondes appartiennent à la vitalité" (*Bulletin de la Société chimique de Paris*, vol. xli. p. 210, March 5, 1884). Fig. 9a represents microscopical slides of pure salicylic acid crystals deposited from alcohol and ether.

#### XI. THE TREATMENT OF PHTHISIS BY INJECTION AND OTHER METHODS.

Before I come to my own experiments, I wish to allude to the work of others in the same direction.

\* Dumas, *Comptes Rendus*, vol. lxxv. p. 295; Bouchardat, *Annales de Chimie et de Physique* (3rd series), vol. xiv. p. 61; Griffiths, *Proc. Roy. Soc. Edin.*, vol. xiii. [No. 121], p. 527.

In the *British Medical Journal* for December 18, 1886, there is an article from the pen of Dr J. H. Bennet (of Paris) on Dr Bergeon's treatment of pulmonary phthisis by means of anal injections of two gases. Bergeon found that sulphuretted hydrogen and carbon dioxide gases were absorbed by the intestines without any poisonous effects. He uses the natural "Eaux Bonnes" water from the Pyrenees as his source of pure sulphuretted hydrogen,—and by repeated anal injections of these gases has cured the worst cases of pulmonary phthisis and other pulmonary diseases (see *Comptes Rendus*, July 12, 1886, p. 176, and *Bulletin de l'Académie de Médecine*, 2nd ser., vol. xvi.); and Dr M'Laughlin (Physician of the Philadelphia Hospital) recently reports the cure of thirty

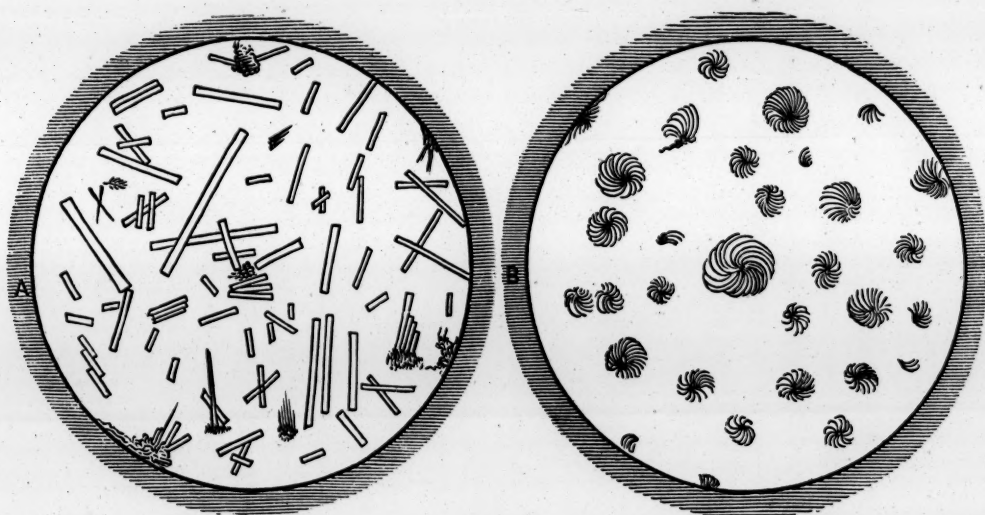


Fig. 9a.

Salicylic Acid Crystals, crystallised from alcohol.  $\times 340$ .

Salicylic Acid Crystals, crystallised from ether.  $\times$  about 95.

patients in the last stages of consumption, solely by using Bergeon's method. Bergeon has found that sulphuretted hydrogen, prepared from any other source than "Eaux Bonnes" water or carbon disulphide, will prove a failure. He "does not propose his method as a microbicide treatment, but merely as one that succeeds," and that "the injection of sulphuretted hydrogen is decidedly antiseptic and curative of local lesions."

Therefore, Bergeon's treatment of phthisis is by anal injection of several litres of the mixed gases into the intestines. There the gases are absorbed into the *venous* system, and pass out by the



lungs. The "antiseptic" gases do not pass into the arterial system, as they would by inhalation.

Through the kindness of Mr Snodgrass (already mentioned), allowing me to make free use of the important letters he has written me, I am in a position to give critical opinions on the treatment of acute general phthisis by means of—

- (1) Hypodermic injections of warm solutions of salicylic acid.
- (2) Bergeon's anal injections of antiseptic gases.
- (3) Inhalation of volatilised iodine.
- (4) The inhalation of, and hypodermic injections of Eucalyptus oil.

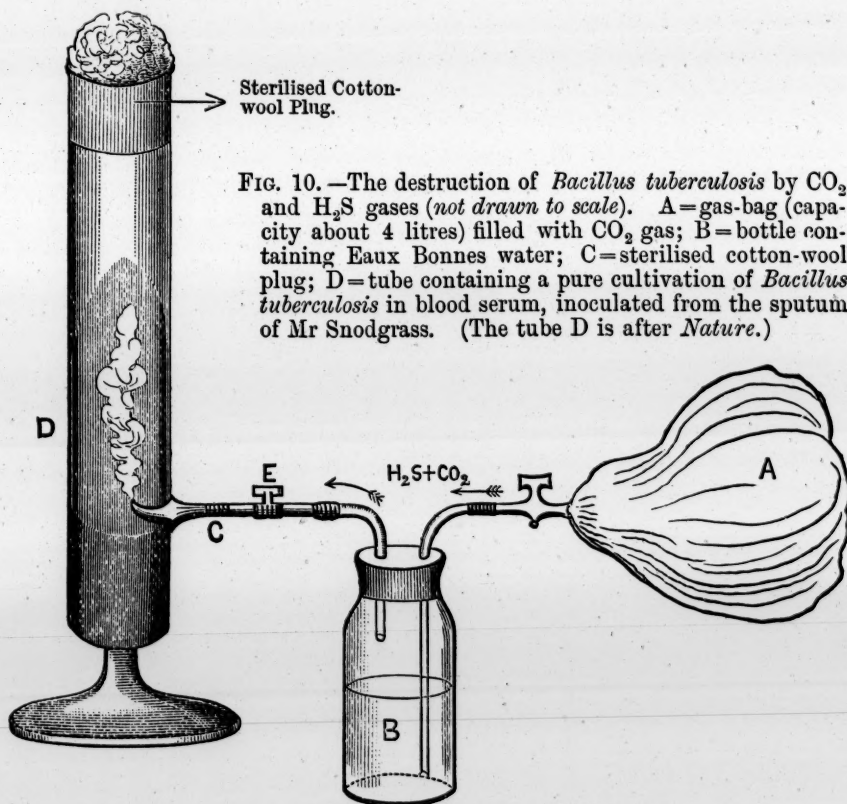


FIG. 10.—The destruction of *Bacillus tuberculosis* by CO<sub>2</sub> and H<sub>2</sub>S gases (*not drawn to scale*). A=gas-bag (capacity about 4 litres) filled with CO<sub>2</sub> gas; B=bottle containing Eaux Bonnes water; C=sterilised cotton-wool plug; D=tube containing a pure cultivation of *Bacillus tuberculosis* in blood serum, inoculated from the sputum of Mr Snodgrass. (The tube D is after *Nature*.)

Dr Bergeon gives no details of having studied the action of his gaseous antiseptics on the vitality of *Bacillus tuberculosis*, therefore it is on this point my next remarks will be directed. Bergeon recommends the anal injection of 4 litres of the mixed gases (see his paper, *loc. cit.*); the time prescribed for injecting this quantity is 20 minutes. I prepared pure carbon dioxide (from the decom-

position of pure sodium bicarbonate by means of pure dilute sulphuric acid) and filled a 4-litre bag with the gas. This gas was passed through a half-bottle of Eaux Bonnes water (thoroughly impregnated with the  $H_2S$  gas), and then allowed to pass into a pure cultivation of *Bacillus tuberculosis* (fig. 10). After all the gases had passed through the cultivation, the tap F. (fig. 10) was turned off. Ten tubes containing sterilised blood serum were inoculated from the growths which had been submitted to the

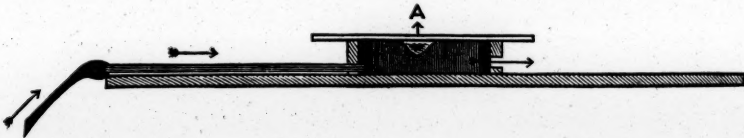


FIG. 11.—Action of  $H_2S$  and  $CO_2$  gases directly upon the bacilli in fresh human sputa. A=a drop of human sputum adhering to the cover-glass.

action of the gases. The tubes so inoculated were then placed in the incubator at a temperature of  $37^\circ C$ . After forty days' incubation, no signs of any growths made their appearance in any of the

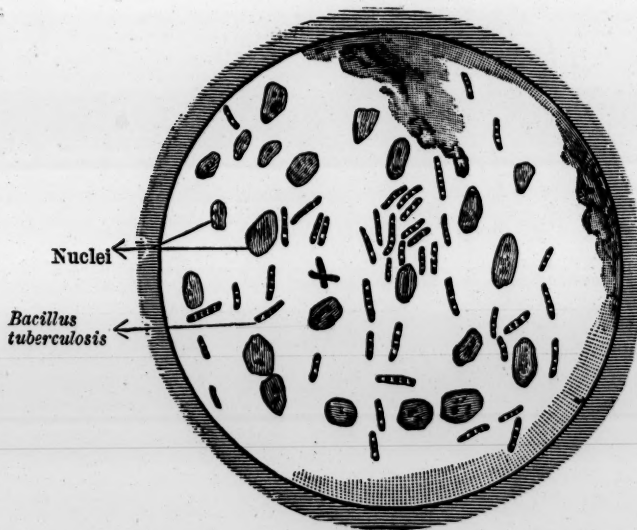


FIG. 12.—Bacilli in Sputum. Case of Miss Green-White. Stained by the Koch-Ehrlich Method.  $\times 750$ .

tubes. These experiments were repeated a second time with similar results.

Again, the gaseous antiseptics were allowed to pass for 15 minutes

into a little glass cell (fig. 11), containing upon the internal surface of the cover-glass (A) a drop of sputum.

After allowing the gases to pass through the little cell, the cover-slip was then transferred to sterilised blood serum, and after an incubation of twenty-six days *no* growths of the *Bacillus tuberculosis* (or putrefactive microbes) made their appearance. This experiment was repeated in duplicate with the same results.

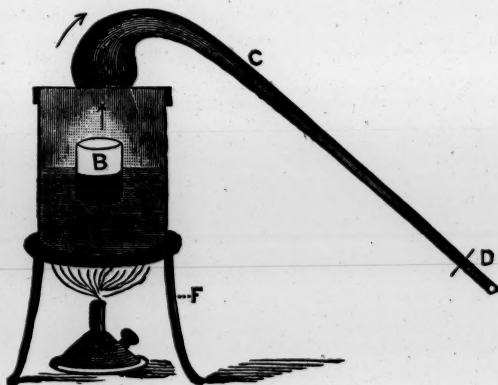
The sputa used were obtained from Mr Snodgrass and Dr Wood of Bromsgrove. Dr Wood's tube came to me labelled "*Expectorated, May 29, 1887. Girl named Miss Green-White. Incipient phthisis; night sweats, and harsh breathing under the clavicles.*" An examination of this specimen of sputum gave numbers of bacilli (fig. 12).

From the above experiments, I have reason to conclude that Bergeon's sulphuretted hydrogen gas is a destroyer of the vitality of *Bacillus tuberculosis* and its spores.

(a) *The Case of Mr Snodgrass. A practical Trial of the Bergeon and Griffiths' Methods of treating Phthisis.*

Mr John Snodgrass, jun., of Glasgow, wrote to me in February of the present year (1887), after reading an abstract of my paper (read before your Society on January 31, 1887) in the *Glasgow*

FIG. 13.—A=a tin vessel containing water (which is kept near its boiling point); B=a small vessel capable of floating in water. This vessel contains tincture of iodine ( $\frac{1}{2}$  oz. of tincture of iodine and  $\frac{1}{2}$  oz. of water are used every time); C=tube (of indiarubber); D=mouthpiece; E=spirit-lamp; F=a tripod stand.



*Herald*, and from that day a scientific correspondence has been kept up between us. His case is that of lung disease of thirteen years' standing, which became distinctly tubercular several years ago. Ever since the discovery of Koch's bacillus, Mr Snodgrass has tried various devices (of his own) for destroying the microbes in his own

lungs. Amongst these experiments he has used volatilised iodine in the following way:—The apparatus (fig. 13) explains itself, and is very simple.

According to Mr Snodgrass, the patient should, if possible, inspire gently by the mouth (from the mouthpiece D) and expire by the nose, taking as full and as deep inspirations as possible. He considers that although the iodine may not reach very deeply into the lungs, it will cleanse the throat, larynx, trachea, and the large bronchi. Concerning the value of his device of inhaling volatilised iodine, he says—“*The inhalations of iodine have certainly put the hand back on the dial in my case for nearly two years.*”

Concerning the adoption by Mr Snodgrass of Dr Bergeon's and also my process of hypodermic injection of a solution of a salicylic acid, I have his permission to make free use of his letters, in which he describes the experiments performed and results obtained. Abstracts from these letters I shall give as an appendix to this memoir. By so doing, it will make the paper far more valuable (as they come from a literary man, and a man of sound common sense) than any written description I could give of his trial of the two methods.

Mr Snodgrass firmly believes in the value of both methods, and much good has been done by using them. When he wrote to me in February (1887) he was apparently a dying man. He greatly improved by using the methods; in fact, so much so that he was able to leave Glasgow and spend the summer in the Kyles of Bute. During his experiments, I have reported on many occasions the microscopical appearances of specimens of sputa received from him, and it was surprising to note from time to time the decreasing numbers of bacilli present. Although there appears, in the last specimen of sputum received on 28th September 1887, to be an increase in their numbers (due to the fact that Mr Snodgrass has for a short time desisted from using the methods, owing to great physical weakness), there was no *increase* in the quantity of Freund's cellulose in the sputum; showing the inactivity of the bacilli present. In fact, their “pathological power” appears to be proportional to the quantity of cellulose found in the sputum.

At this point I will refer you to the appendix of this paper, where Mr Snodgrass states in his own words the work that has



been done to prove that if consumption is curable at all, it must be done by injection methods of some germicide, either in the gaseous or in the liquid condition. It will be remembered that the concluding words of my first paper on this subject (*Proc. Roy. Soc. Edin.*, vol. xiv. p. 97) were the following:—

“I have reason to conclude that it may be, with a more extended study of the action of this solution of salicylic acid upon disease ‘germs’ and their organisms, we have the most rational mode of treating those contagious diseases whose seat of energy is in the blood.”

Perhaps a better germicide than salicylic acid may be discovered, yet it is the principle of the method that is so important. In every case of disease produced by living microbes residing in the blood the most rational and scientific method of treating such diseases would be to destroy the microbes *in situ* by injections. When the microbes are destroyed, nature will have a chance of repairing the damage done. It is with this end in view, that forms the basis of the present researches, and of my method for treating contagious diseases.

The germicides used in connection with my experiments upon *Sarcina lutea*, *Micrococcus prodigiosus*, *Micrococcus tetragonus*, all destroy *Bacillus tuberculosis*. In fact, I may say that Mr Wm. Thomson’s\* sodium fluosilicate is a very powerful germicide. According to Mr Thomson, F.R.S.E., it is not poisonous, and is inodorous. A saturated aqueous solution contains 0.61 per cent. of the salt. It does not irritate wounds, and it has “greater anti-septic power for animal tissues than one part of mercuric chloride in 1000 of water: which is a stronger solution than that which can be generally employed for surgical purposes without producing poisonous effects” (Thomson).

(b) *The Kolischer Treatment of Consumption.*

In passing I wish to record here, that in June 1887, there were accounts given in the newspapers that Dr Kolischer had recently presented a paper to the Vienna Society of Physicians, on a proposed method for treating and curing consumption and other

\* A paper read before the British Association, August 1887, and published in the *Chemical News*, vol. lvi. p. 132.

tubercular affections of the lungs, or other parts of the body. Dr Kolischer, starting on the assumption that tuberculosis occasionally heals naturally, owing to the tubercles being "calcined," hit upon the idea of causing artificial "calcination" by means of *hypodermic injections* of a substance described as *calcium phosphoricum*, into the limbs of persons affected with tuberculosis. He made a number of experiments with a view to testing his discovery, and in every case the experiments turned out successful.

(c) *M. Ball's Treatment of Phthisis by Injections of Eucalyptus Oil.*

Recently M. Ball (Membre de l'Académie de Médecine de Paris) read a paper before the Paris Academy of Medicine, stating what he considers to be a cure for consumption, namely, by *injections* of eucalyptus oil under the skin. I may say here, that Mr Snodgrass has used eucalyptus oil volatilised by heat and inhaled; and he says:—"It proved very irritating, and I had to desist. I greatly prefer salicylic acid injections to it."

(d) *Dr Theodore Williams' Observations on the Influence of certain Substances on the Growth of Bacillus tuberculosis.*

In a paper,\* kindly sent to me by the author, there are detailed a number of experiments with different reagents on the bacillus of phthisis. Dr Williams found that arsenious and boric acids "*exercised no destructive influence on the bacilli*" of consumption. He found that, with solutions of quinine sulphate (2 grains to 10 grains in an ounce of water) in each case the number of bacilli decreased rapidly under its influence, and "*that the bacilli in the sputum after being mixed with the quinine salt could not be cultivated even in beef-broth.*" The experiment shows that quinine sulphate is a destroyer of *Bacillus tuberculosis*. Dr Williams also found that iodine (1 part in 12 of water) reduced the numbers of bacilli, and prevented spore-formation. Mercuric chloride (1 grain to an oz. of water) caused no diminution, but rather an increase of bacilli-spore-forma-

\* "Observations on the Influence of certain Culture Fluids and Medicinal Reagents in the Growth and Development of the *Bacillus tuberculosis*," by C. T. Williams, M.A., M.D., F.R.C.P., Physician to the Hospital for Consumption, Brompton, *Proc. Roy. Soc.* [No. 231], 1884.

tion was very marked in the mercury solution. This fact confirms Herroun's investigation on the value of mercuric chloride as an antiseptic agent.

## XII. *Bacillus tuberculosis* A PARASITE.

From Dr V. Cornil's researches (*Bulletin de l'Académie de Médecine de Paris*, 1883) it has been shown that *Bacillus tuberculosis* is of a parasitical nature. The microbe is found in the giant cells of the tubercle, also in the colourless blood corpuscles; and therefore it is to be detected in all organs in which a tubercle can be developed. It passes to the kidneys, and according to Babès (*Centralblatt für d. Med. Wissensch.*, 1883, p. 145) has been found in the urine. Recently, M. V. Galtier (*Comptes Rendus*, vol. civ. No. 19) has shown that whey and cheese from the milk of tuberculous cows often contain the bacilli of phthisis. He has also demonstrated that swine and poultry fed upon dairy produce of this character may contract phthisis. Their flesh may then in turn impart the disease to man. Galtier's observations appear to explain the hereditary nature of consumption. It is apparently a blood disease of slow growth; and if milk be a medium in which the bacillus is capable of living its life-history, we can well understand a phthisical mother suckling her child giving the disease to the child.

It will be remembered that Dr Klein has proved the presence of *Micrococcus scarlatinæ* (*The Times*, 28th May 1887) in the milk of those cows suffering with certain diseases of the udders and teats.

Therefore, if milk is a nutritive medium for infectious diseases, it would be better in every case to nearly boil the milk before using it.

## CONCLUSIONS.

1. It has been proved beyond doubt that microbes are the real cause of certain contagious diseases.
2. In many cases these microbes are capable of being destroyed by various germicides. Therefore, by further investigations, we ought to discover a germicidal remedy for such terrible scourges to humanity as consumption and syphilis.

3. It has been shown from the researches detailed in this paper that the vitality of *Bacillus tuberculosis* is considerable, and that it is capable of being dried up in the atmosphere for many weeks without its vitality being impaired.

4. That *Bacillus tuberculosis* is capable of being disseminated by envelopes coming from phthisical patients.

5. That the electric current destroys the vitality of certain microbes.

6. That a new bacterium is the cause of putrefaction in the onion, liberating as a product of its life-history small quantities of  $H_2S$  gas. This new microbe I have ventured to call *Bacterium allium*.

7. That the soluble zymases secreted by living microbes are capable of being destroyed by germicidal agents. Hence, if destroyed, they are incapable of producing chemico-pathological changes in the blood and tissues.

8. The most rational method of treating contagious diseases is by injection of some germicidal agent, either in solution or in the gaseous state. By destroying the microbes, the disease would be at an end.

9. The germicidal agents used for injection purposes must *not* produce poisonous actions upon the blood and tissues, yet at the same time must be powerful enough to destroy the vitality of the microbes and their spores.

10. It is upon the lines indicated in this memoir that the physician in the future must look for a scientific method of treating those contagious diseases whose microbes reside in the blood.

I wish to tender my best thanks to those friends who have rendered me assistance during these investigations, but more especially to Mr John Snodgrass and Dr Wood.

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#### APPENDIX.

The following are abstracts from Mr Snodgrass' letters :—

(1) *Letter of 28th February 1887.*—"Some time since I obtained remarkable results by a simple process I devised for the inhalation of the volatilised vapour of iodine, with satisfactory results."

(2) *Letter of 3rd March 1887.*—"The result of inhalation of



iodine three days ago has been to cleanse the lung, and to bring away the débris found in the sputum this forenoon. This is, of course, a good result. The iodine inhalation caused headache and considerable depression of heart."

(3) *Letter of 8th March 1887.*—"Since I last wrote to you I have twice injected a 15 minim solution of the acid. The strength was  $2\frac{1}{2}$  grains of salicylic acid to a fluid drachm of water; but the acid was mixed with an equal quantity of borax. My immediate reason for injecting the solution (which would roughly contain  $\frac{5}{8}$ th of a grain of the acid) was a severe attack of rheumatism, of the kind that often accompanies phthisis. The rheumatism disappeared almost entirely. . . . I may mention, that before making the injections there was a large deposit of uric acid on the urine. This has quite disappeared, at least to the naked eye. That the salicylic acid passed through the system I am perfectly certain, as I had the usual headache which follows taking it by the mouth, and the taste—that unmistakable taste—was very apparent next morning on the tongue and palate. I made the injections in the calf of the leg, near a large vein. . . . I had (it appears) rightly assumed that a cavity was forming in the lung about the time I first wrote to you. About forty-eight hours after inhaling volatilised iodine a considerable quantity of matter came away, with the usual discoloured blood-clot. This débris on examination, contained an abundance of long fibre. . . . One most important part of your paper is, that which deals with the action of the gastric juice on medicines. I swallow a great deal of sputum at times—for it is impossible always to eject the whole. Yet I have every reason to suppose that the bacilli thus swallowed in large numbers *pass harmlessly through the alimentary tract without getting into the blood*. In fact, it happens with me and *Bacillus tuberculosis*, as it happened with M. Bochefontaine and the *Comma bacillus*. This thoroughly bears out your most important remark that, 'there is no doubt the *acid* properties of the gastric juice . . . had acted upon these micro-organisms,' &c. In cases where consumption of the intestines follows upon pulmonary consumption, the inference will be that the gastric juice is either weak or imperfectly secreted. With reference to the salicylic acid (without borax), what I think of doing is to dissolve a part of the acid in a

small quantity of hot water; then, if the water takes up the acid in the proportion of 20 to 1, by injecting 15 minims of the solution before it is cold—say at the temperature of blood heat—I shall get into the system about  $\frac{3}{4}$  of a grain of the acid. Now, the medium dose by the mouth being 10 grains,  $\frac{1}{3}$  of this would be reckoned safe, or at least not dangerous by injection, consequently I am much within the line of safety.”

(4) *Letter of 11th March 1887.*—“This forenoon I tried the injection of salicylic acid, and after injecting 5 or 6 minims into the tissue of the left thigh, I had to stop, owing to the pain caused by the acid. Judging by the pain that immediately followed the injecting of the drops of fluid, the solution must have been of considerable strength. The only question is, whether the heat of the body is sufficient to dissolve any crystals that remained in the fluid? I suspect that this is the great fallacy of administering (say) 10 grain doses of salicylic acid by the mouth. Possibly very little of the acid passes into the blood system, the greater part being carried away in the fæces as insoluble. From this, I am sure your method is on the right lines. The micro-organisms *must be reached and must be destroyed.*”

At this point Mr Snodgrass uses Dr Bergeon’s method along with the salicylic acid injections.

(5) *Letter of 28th March 1887.*—“Dr Bergeon’s instrument (the only one in Scotland) has been seen by my doctor. It is very elaborate; we think needlessly so. Briefly, the mode of obtaining the mixed gas is to pass the carbonic acid gas through Eaux Bonnes water. Now, it seems that during five or six trials on a patient at the Western Infirmary (Glasgow) no sulphuretted hydrogen could be detected being emitted through the mouth, showing that the gas did not permeate the lungs. My notion is that too little Eaux Bonnes water was used, that a fresh supply should from time to time have been put into the jar in which the  $\text{CO}_2$  passed through the water. Your suggestion of the proportion of three volumes of  $\text{CO}_2$  to 1 of  $\text{H}_2\text{S}$  is very valuable.”

Mr Snodgrass and his doctor construct a much simpler apparatus for this gaseous injection than that of Bergeon.

(6) *Letter of 31st March 1887.*—“At this point, I may say extensive damage has been done to the throat, for large ulcers are

seen on the back of it, by merely pressing down the tongue. Two litres of gas ( $\text{CO}_2$  and  $\text{H}_2\text{S}$ ) were injected, and the sulphur smell was distinctly perceptible five minutes after beginning the injection. Eaux Bonnes water was not used, as it is difficult, but bisulphide of carbon was employed, instead of the mineral water. Some rather disagreeable symptoms followed the injection; severe headache, colic pains, weakness, and slight pain of the heart. You will be glad to learn that (now for the third time) relief from rather severe rheumatism followed two injections of salicylic acid."

(7) *Letter of 5th April 1887.*—"After the last injection of gas, I again suffered from severe toxic effects, and during the night was much pained with the unabsorbed gas, and with violent and incessant purging. . . . Many persons would be far too weak for Bergeon's method, and for them your salicylic acid treatment would be invaluable, for I have not the least doubt that you have discovered an efficient bacillus-destroyer."

(8) *Letter of 10th April 1887.*—"To-day I have injected 10 minims of salicylic acid solution. It certainly 'bites' pretty sharply, so it must have been strong enough. I find it of advantage to inject it tepid, as it is then more quickly absorbed; the swelling going down in a few minutes. There is no doubt that I am much better, as far as phthisical disease is concerned. I can now speak without distress, and breathing is much less laboured. Last night I slept seven hours, a thing that has not happened for months, my usual sleep being a broken half hour several times a night, and in all not more than three hours in the twenty-four. I firmly believe that in many cases Dr Bergeon's system will not be applicable, and in these cases your treatment would be valuable."

(9) *Letter of 13th April 1887.*—"Yesterday I again injected the gases. They produced much pain in the left lung, and also in that small portion of the right lung that is impaired. My doctor thinks your observations of immense importance."

(10) *Letter of 27th April 1887.*—"For the last ten or twelve days I have suffered a great deal of pain. Large quantities of uric acid are being secreted. I inject salicylic acid occasionally, which has the effect of checking the formation of uric acid."\*

\* See "On some Points in the Pathology of Rheumatism, Gout, and Diabetes," by Dr P. W. Latham (*The Croonian Lectures for 1886.*)—(A.B.G.)

In the month of June Mr Snodgrass was well enough to travel to the Kyles of Bute from Glasgow. He writes from there as follows :—

(11) *Letter of 17th June 1887.*—"My lung trouble has of late *very greatly* improved ; but the abdominal mischief has been very severe. During the last two days, however, a marked improvement has taken place. . . . I have the greatest faith in yours and Dr Bergeon's systems. But, in my case the disease has apparently gone too far, and the condition of the large bowel is such that there is great risk in applying Bergeon's method."

(12) *Letter of 19th September 1887.*—"You will recollect that you were able to make a most favourable report on the sputum sent to you after the salicylic acid treatment and eight or nine injections of gas by the Bergeon mode of treatment. As far as the lung was concerned, a *great improvement* took place, and for more than two months—certainly during the whole of June and July—I could *not have sent you a typical specimen of sputum*. Indeed, during that time expectoration almost entirely ceased (as did also the cough), and what there was, was merely mucous phlegm, such as might be present in a slight attack of bronchial inflammation. Otherwise, however, matters were very bad ; the large bowel was severely ulcerated, and adherent in the ileocecal region to the wall of the abdomen. I continued to inject salicylic acid after ceasing the Bergeon treatment, but this too had to be discontinued on account of the disordered state of the whole system. One remarkable thing, however, has occurred,—*ever since the salicylic acid injections I have had no attack of muscular rheumatism*.

"About a month ago, I again began the Bergeon treatment, but very cautiously, and the operations have been continued. There is undoubtedly some improvement, but, as before, *uric acid deposits took place*, and I have not ventured to use the treatment on more than two days consecutively.

"I am afraid it must be admitted that (with me at least) there are certain dangerous symptoms caused by the CO<sub>2</sub>. Amongst these are, obstinate constipation, great difficulty in expelling the unabsorbed residuum of carbonic acid gas in the intestines, and certain rather alarming head symptoms.

"About the 23rd of June last, Dr Coghill, of Ventnor, com-



municated an account of his experience of the Bergeon treatment to the *British Medical Journal*. He there stated that the results, both in his hospital and in his private practice, were not merely remarkable, but astonishing. In the same number of the *British Medical Journal*, however, a physician of one of the London hospitals says that not only negative results, or but very imperfect results, were obtained from experiments in the hospital he represents. But this scarcely surprised me after the failure at the Western Infirmary (Glasgow). Most likely Eaux Bonnes natural mineral water was used. I found it quite inefficient. Again, in the same journal, another communication gives, as a formula from which excellent results had been obtained, the following:—a saturated solution (aqueous) of washed sulphuretted hydrogen;  $\frac{1}{2}$  to 2 oz. being added to 12 oz. of pure water in the bottle through which the stream of carbon acid gas is passed."

It will be gathered from the experiments of Mr Snodgrass and those of my own—

1. That inhalation of iodine vapour has the property of cleansing the lungs, &c., of bacilli, débris, and Freund's cellulose.
2. That both the salicylic acid injection method, and Dr Bergeon's treatment, are capable of preventing the growth and multiplication of *Bacillus tuberculosis*. So much so, that Mr Snodgrass was comparatively free from the phthisical complaint for two months; although the disease is of *long-standing*, and there are little hopes of a permanent cure.
3. That Bergeon's process has a tendency to greatly increase the formation of uric acid in the urine.
4. That salicylic acid injections lessen the abnormal formation of uric acid in the urine.
5. That in severe cases of phthisis, it is difficult for the whole of the gases (in Bergeon's treatment) to be absorbed by the intestines, —the unabsorbed gases causing an ulcerated state of the intestines.
6. That salicylic acid *injections* have the power of completely curing muscular rheumatism of the kind which often accompanies phthisis.

8. On the Colour of the Skin of Men and Animals in India. By Robert Wallace, *Professor of Agriculture and Rural Economy in the University of Edinburgh.*

*Indian cattle* have, with few exceptions, jet black skins. The hair is frequently white, grey, brown, or black, but only in rare cases are the skins white. The animals having white skins are weakly, if not unhealthy, are liable to blister with the sun, and contract after a time a form of leprosy.

*Black Skin an Advantage.*—This opens up and widens the scope of a most interesting question on the relation of colour to climate, which—I have it on the very highest authority, that of Professor Huxley—is by no means at present understood. The field of investigation as regards India is a large one; it embraces the human races, and the breeds of cattle, sheep, pigs, buffaloes, and horses. The skins of all, as a rule, are black or dark coloured; the few white exceptions I have noticed particularly in buffaloes and cattle, and in one case of a goat, are as stated delicate. The white or grey hair so prevalent in cattle extends to the Arab horse, and would appear to be, when associated with the black skin, peculiarly well adapted to resist the extreme heat of a tropical sun. It has always been a marvel that the white skin, which on account of its colour does not absorb heat so quickly as a black skin, should not prevail in the human species within the tropics; and it becomes even more wonderful now, when it begins to dawn upon us, that the skins of the lower animals follow the same great law of nature, whatever that law may be.

*Black Skin Theory Explained.*—It would seem at first sight that the black skin should rather be a disadvantage than otherwise; but in the reality it is not so. The black colour of the skin causes it to absorb more heat than a white skin, but while it is doing so, at the same time and for the same reason, it is giving off more heat—its absorbing power and also its radiating power being greater. Therefore, when the sun's rays impinge upon the skin, the heat is rapidly absorbed; but, as the rate of absorption of heat is greater than the rate of radiation, unless the temperature of the skin were lowered by some other influence, the whole surface of the body would become extremely hot.

To complete the explanation, we must here take into consideration what is known of black-skinned men. Any one who has been in India can see that natives, although they drink water freely, do not appear to perspire so copiously as Europeans, but this is simply because more of the perspiration comes from them in the form of vapour, and less is seen to stand like dewdrops on the surface of the skin. In the evaporation of the moisture exuding from the skin, we have a demand for heat far greater than an ordinary observer might imagine; and by it can be disposed of all the surplus heat which the black skin absorbs over and above what it gives off by radiation. It is a fact which few realise, that the amount of water is small indeed which, by being evaporated, could transform into its latent condition all the heat derived from the warming influences of the sun in the hottest climates.

PRIVATE BUSINESS.

Mr D. S. Sinclair, Dr A. D. Leith Napier, and Mr Alexander Galt were balloted for, and declared duly elected Fellows of the Society.

*Monday, 19th December 1887.*

SIR DOUGLAS MACLAGAN, M.D., Vice-President,  
in the Chair.

The following Communications were read:—

1. On the Height and Volume of the Dry Land, and the Depth and Volume of the Ocean. By John Murray, Esq., Ph.D. (Published in the *Scottish Geographical Magazine* for January 1888.
2. The Pineal Body (*Epiphysis cerebri*) in the Brains of the Walrus and Seals. By Prof. Sir Wm. Turner, M.B., LL.D., F.R.S.

In this paper the author described the pineal body in the walrus and in *Phoca vitulina* and *Macrorhinus leoninus*, in which animals, but more especially in the walrus, it is of larger size than is usual in mammalia. In one walrus it measured 30 mm. (1·18 inch) in

length and 18 mm. (0·7 inch) in its greatest transverse diameter; in another it was 29 mm. long, 13 mm. broad, and 13 mm. in vertical diameter. It rested on the superior vermiform process of the cerebellum, and was visible between the two diverging hemispheres of the cerebrum when the brain was looked at from above. [The paper is printed *in extenso* in the *Journal of Anatomy and Physiology*, January 1888, and as a part of the "Report on the Seals" collected by H.M.S. "Challenger," Part LXVIII., 1888.]

**3. On a Method of graphically recording the exact Time Relations of Cardiac Sounds and Murmurs.** By Byrom Bramwell, Esq., M.D., and R. Milne Murray, Esq., M.B.

(Printed in full in *Brit. Med. Journal*, Jan. 7, 1888.)

**4. On Benzyl Phosphines.** By Professor E. A. Letts and W. Wheeler, Esq.

The phosphines have, comparatively speaking, been little studied, and most of our information concerning them is due to the investigations of only two or three observers. There are consequently many points in their history which require examination, and the object we had in making the present research was to extend our knowledge of them as a group. We experimented in the benzyl series for several reasons, among which we may mention the following:—

Monobenzyl phosphine is a liquid at ordinary temperatures, whereas the corresponding methyl and ethyl derivatives are gaseous, hence it is more easily worked with than the latter. Then, again, benzyl derivatives have, as a class, considerable chemical activity; and lastly, one of us in conjunction with another chemist had already studied somewhat exhaustively the quaternary phosphorised compounds which this radical forms.\*

Hofmann † was the first to obtain mono- and di-benzyl phosphine, by heating a mixture of chloride of benzyl, phosphonium iodide, and oxide of zinc in sealed tubes. He apparently submitted them to a somewhat cursory examination, and only determined their leading

\* Letts and Collie, *Trans. Roy. Soc. Edin.*

† Hofmann, *Ber. d. d. Chem. Ges.*



properties. He mentions that bye-products are formed along with them which he did not further investigate.

We have repeated Hofmann's experiments, and have submitted both the mono- and di-benzyl phosphine to a very careful examination. We have also investigated the bye-products which are formed and have determined as far as possible their composition.

*Preparation of Mono- and Di-Benzyl Phosphine.*—Hofmann recommends digestion during six hours at  $160^{\circ}$  C. of a mixture of 4 parts of oxide of zinc, 16 of iodide of phosphonium, and 12 of chloride of benzyl. Experiments conducted in this way with commercial chloride of benzyl from Kahlbaum gave in the tubes a viscous semicrystalline mass. To obtain a good result, thorough mixing of the materials in the sealed tubes by shaking before heating seemed to be necessary. On opening the tubes much phosphuretted hydrogen escaped, but on heating for a longer period or to a higher temperature, the escaping gas *seemed to consist of hydrochloric acid only*. It was soon found that at the temperature of  $160^{\circ}$  C., a great deal of hydrochloric acid is formed, and but little of the primary phosphine. The best results were obtained by a six hours' digestion of the mixture at temperature of  $120^{\circ}$  C. Experiments tried at  $100^{\circ}$  to  $110^{\circ}$  C. showed that but little of the primary phosphine is formed.

With the quantities Hofmann recommends and a digestion for six hours at  $120^{\circ}$ , the tubes when cold contain a viscous semi-transparent mass, sometimes of a brown colour, sometimes red and opaque from the separation of free phosphorus. Above this a small quantity of a liquid usually floats, which at times is mobile, but at others thick and slightly fluorescent. A few crystals of undecomposed iodide of phosphonium are also frequently present.

The liquid floating on the viscous mass (consisting of benzyl chloride, toluol, &c.) we usually poured off, whilst the viscous mass itself we removed by inverting the tubes and blowing a current of steam through them—the operation being so conducted that no air was admitted, whilst the viscous mass (which liquefies when warmed) was allowed to run into a distilling flask—without coming in contact with the air.

The crude primary phosphine (liberated by the action of water on the product of the reaction) was then distilled off in a current of

steam, a stream of carbonic acid passing through the apparatus to prevent oxidation.

Under favourable circumstances, from 60 to 70 grms. of the impure primary phosphine were obtained from 360 grms. of benzyl chloride.

The residue in the distilling flask usually consisted of a slightly brown viscous mass, which solidified on cooling. It contained the secondary phosphine and several bye-products. The water in the distilling flask along with it also contained phosphorised bodies.

After various experiments, we found that the best method for isolating the secondary phosphine is as follows :—

The mass is boiled several times with water until the latter ceases to extract zinc salts. It is then boiled with strong potash solution, which removes a further quantity of zinc salts, and after washing with water it is extracted with boiling spirit, in which most of it dissolves ; leaving, however, a small quantity of a black viscous syrup.

The alcoholic solution on evaporation yields crystals of crude dibenzyl phosphine, which may be purified by recrystallisation from spirit. The mother liquors on further evaporation yield, in addition to more of the crude dibenzyl phosphine, a viscous substance which contains phosphorised bodies.

In addition to mono- and di-benzyl phosphine we obtained the following bye-products :—

(A) A crystalline substance precipitated on addition of hydrochloric acid to the potash solution used to extract the viscous mass containing dibenzyl phosphine.

(B) A viscous substance remaining in the alcoholic mother liquors after the di-benzyl phosphine had crystallised out.

(C) A solid substance separating from the aqueous solution obtained by treating the contents of the sealed tubes with water.

(D) A crystalline zinc salt also contained in the aqueous solution, from which (C) had been separated.

In this paper we shall first discuss the properties of mono- and dibenzyl phosphine, and afterwards the nature of the bye-products.

*Monobenzyl Phosphine.*—Hofmann purified the crude phosphine obtained by distilling the contents of the sealed tubes with water by fractional distillation only. He states that, after two distillations in

a stream of hydrogen, the phosphine is obtained of the constant boiling point,  $180^{\circ}$ .

Our own experiments, repeated again and again, and with the greatest care, have satisfied us that the pure phosphine cannot be obtained thus readily. In our attempts to obtain it by simple fractional distillation, we operated altogether upon 50 to 60 grms. of the crude substance. The phenomena observed were much the same in each case. The crude products began to boil at about  $100^{\circ}$ . The thermometer then rose rapidly to  $160^{\circ}$ . From  $160^{\circ}$  to  $170^{\circ}$  most distilled, whilst from  $170^{\circ}$  to  $190^{\circ}$  very little passed over. The residue in the retort usually decomposed suddenly above this temperature, with separation of red phosphorus. All the fractions contained the primary phosphine, for they all had its powerful and characteristic odour, and when mixed with fuming hydriodic acid they all gave the crystalline hydriodate. On repeatedly redistilling them no substance of constant boiling point could be obtained. Considering that the boiling point of chloride of benzyl is  $177^{\circ}$ , and that much of that body is contained in the crude phosphine, it is not surprising that mere fractionation fails to separate it from a body boiling only a few degrees higher.

Eventually we decided to separate the phosphine from the crude product by obtaining its crystallised hydriodate, but owing to the bulky nature of that compound and to its insolubility, we experienced considerable difficulty in effecting this. After several experiments, we found that either of the two following methods may be employed :—

(1) The crude phosphine is placed in a retort and a stream of perfectly pure hydriodic acid gas (dried by passing over phosphoric anhydride) is conducted by a long tube into the body of the retort. As soon as saturation seems to be complete the retort is placed in an oil bath, and heated to a temperature of  $160^{\circ}$  to  $180^{\circ}$ , a very slow current of hydriodic acid passing all the time. The hydriodate then sublimes in beautiful colourless scales, and when most has thus volatilised into the neck of the retort, the latter is allowed to cool—the hydriodate shaken out, and well-washed with pure benzol.

(2) The crude phosphine is mixed with about twenty times its volume of pure dry benzol, and the mixture saturated with dry hydriodic acid. It grows warm, and eventually almost solid from

the separated hydriodate. The mass is then thrown on to a linen filter and thoroughly squeezed, then dried between blotting paper, broken up, and washed with benzol so long as the latter dissolves anything. The benzol is then removed by squeezing, drying on filter paper, and exposure of the pounded mass *in vacuo*.

The hydriodate prepared by either of these methods is snow white, and tolerably permanent in air; but if not carefully prepared, it rapidly becomes brown. Its purity was established by a determination of iodine.

The whole of the crystallised hydriodate (about 60 grms.) was placed in a separating funnel, and caustic potash added until the latter was nearly full; the mixture was then shaken, when the hydriodate rapidly decomposed, and the phosphine separated as an oily layer which floated. It was then decanted and submitted to fractional distillation in a stream of hydrogen. The thermometer rose rapidly to  $178^{\circ}$ , then slowly to  $190^{\circ}$ . It was fairly constant from  $180^{\circ}$  to  $182^{\circ}$  when most distilled. Only a little passed from  $182^{\circ}$  to  $190^{\circ}$ . Fraction  $178^{\circ}$  to  $190^{\circ}$  was redistilled. The thermometer rose at once to  $177^{\circ}$ , then rather more slowly to  $178^{\circ}$ . From  $178^{\circ}$  to  $185^{\circ}$  most distilled. The exact boiling point could not be fixed, but most of the liquid distilled from  $180^{\circ}$  to  $183^{\circ}$ .

These experiments, conducted with the greatest care, and repeated two or three times, appear to indicate that monobenzyl phosphine suffers a slight decomposition at its boiling point, which lies somewhere about  $180^{\circ}$  to  $183^{\circ}$  C. (uncorrected).

*Properties.*—Monobenzyl phosphine is a colourless, highly refracting liquid, possessing a very characteristic and penetrating odour. Its smell remains for days on the hands after operating with it, and in one case the smell was observed months after an instrument had been handled by one of us for some time—the fingers having been previously in contact with a trace of the phosphine. Exposed to the air, it at once fumes powerfully, and grows very hot. Its vapour, indeed, often inflames on unstopping a bottle containing it. Mixed with fuming hydriodic or hydrobromic acid, it gives a bulky crystalline precipitate of the haloid salt; and it also, though with more difficulty, gives a hydrochlorate.

*Hydriodate.*—This salt is easily formed either by subliming the



phosphine in dry hydriodic acid gas,—by saturating a solution of the phosphine in benzol with dry hydriodic acid, or by dissolving the phosphine in warm fuming hydriodic acid.

By the first method it is obtained in snow-white scaly crystals, like benzoic acid; by the second, as a seemingly amorphous, bulky precipitate; whilst by the third, it is also obtained in the crystalline state.

A specimen prepared by the first method was analysed—

·6090 gave ·5630 Agl = ·30431 = 49·97 %.

calculated per  $C_7H_7PH_2.HI - 1 = 50·39$  %.

The hydriodate, when pure and dry, is permanent in dry air; but a trace of impurity causes it to become brown. It is rapidly decomposed by water, and instantly by caustic potash solution, the phosphine being set free.

It is very insoluble in benzol, slightly soluble in ether, and sparingly soluble in warm fuming hydriodic acid.

*Hydrobromate*.—Hofmann could not obtain this compound, but we found that it could be prepared with the greatest ease, either by saturating a solution of the phosphine in benzol with gaseous hydrobromic acid, or by dissolving the phosphine in the fuming (aqueous) acid. By the latter method it is obtained in scaly crystals, very similar to the hydriodate.

Its analysis gave a small deficiency of bromine—probably due to slight deliquescence, or to a trace of impurity—

	Obtained.	Calculated for $BzPH_2.HBr$ .
Bromine, . . .	37·9	39·0.

The salt is insoluble in benzol, and only very slightly soluble in warm fuming hydrobromic acid.

It decomposes rapidly in contact with water, and instantly with caustic potash.

*Hydrochlorate*.—Hofmann did not succeed in obtaining this salt, but it may be produced by similar methods to those which we employed for obtaining the two last-named compounds.

On passing gaseous hydrochloric acid into the pure phosphine dissolved in benzol, no effect is produced until the solution is quite saturated. Then white crystalline scales begin to form.

On shaking the phosphine with a saturated aqueous solution of hydrochloric acid, a white crystalline precipitate is produced, which dissolves on shaking or on gently warming.

Owing to our small stock of the phosphine, we were unable to obtain sufficient of this compound for analysis, but there can be little doubt as to its composition.

*Action of Bisulphide of Carbon on Monobenzyl Phosphine.*—We thought it possible that monobenzyl phosphine might react with bisulphide of carbon, so as to give a phosphorised sulphur urea, and the following experiments were accordingly tried:—Two grms. of the pure phosphine were sealed up with 2 grms. of bisulphide of carbon, and heated at 120° C. for two days. On examining the tube after heating, the contents were found to consist of a viscous, colourless substance and a number of colourless needle-shaped crystals. When opened, a considerable quantity of sulphuretted hydrogen escaped.

The contents of the tube were treated with bisulphide of carbon, which dissolved the viscous substance, but left the crystals. The latter were repeatedly washed with the bisulphide, then dried, and submitted to a combustion.

$$\cdot 2175 \text{ gave } \begin{cases} 0\cdot 107 \text{ H}_2\text{O} = 0\cdot 0118 \text{ H} = 5\cdot 42 \% \\ 0\cdot 4335 \text{ CO}_2 = 0\cdot 11823 \text{ C} = 54\cdot 3 \% \end{cases}$$

	Obtained.	Calculated $\text{C}_7\text{H}_7\text{PH}_2\text{S}$ .	$\text{C}_7\text{H}_7\text{PS}$ .
Carbon, .	54·3	53·85	54·54
Hydrogen, .	5·4	5·79	4·5

Owing to the very small quantity of product at our disposal (about 0·25 gm.), we were unable to examine it further; hence its composition must remain doubtful, though its formula is probably either one or other of the two given above.

The bisulphide of carbon washings from the crystals were warmed to get rid of the bisulphide. A slightly yellow gummy mass remained, which was insoluble in water, alcohol, and ether, and from which no definite product could be obtained by the action of various reagents. It was, however, noticed that boiling glacial acetic acid dissolved it to a certain extent, and it was therefore treated with a considerable quantity of this solvent, in the hope that, if it consisted of two or more products, a separation might be effected.

The hot acetic solution deposited on cooling oily droplets, which eventually formed a viscous mass, exactly like the original substance. The acetic solution decanted from this was evaporated to dryness on the water-bath, until the whole of the acetic acid had volatilised. There remained a gummy mass not different in appearance from the original body.

An analysis was made of this portion (*i.e.*, that which remained dissolved in the cold acetic solution), and also of the portion which the acid had not dissolved.

The results show that both portions have the same composition—but no probable formula could be calculated.

(A) *Portion dissolved by Acetic Acid.*

$$0.4355 \text{ gave } \begin{cases} 0.828 \text{ CO}_2 = 0.22582 \text{ C} = 51.8 \% \\ 0.1982 \text{ H}_2\text{O} = 0.02202 \text{ H} = 5.12 \% \end{cases}$$

(B) *Portion not dissolved by Acetic Acid.*

$$0.3643 \text{ gave } \begin{cases} 0.654 \text{ CO}_2 = 0.17836 \text{ C} = 51.5 \% \\ 0.1592 \text{ HO}_2 = 0.1772 \text{ H} = 5.1 \% \end{cases}$$

	Obtained.		Calculated for		
	A	B	(C <sub>7</sub> H <sub>7</sub> )PH <sub>2</sub> .CS <sub>2</sub> .	(C <sub>7</sub> H <sub>7</sub> )PCS.	(C <sub>7</sub> H <sub>7</sub> PH) <sub>2</sub> CS
Carbon,	51.8	51.5	48.0	57.9	62.1
Hydrogen,	5.02	5.1	3.5	4.2	5.5

*Oxidation of Monobenzyl Phosphine by Air.*—As before mentioned, the primary phosphine attracts oxygen with great energy from the air. The temperature rises considerably; dense white vapours are produced, and these occasionally take fire spontaneously. The substance which eventually results is a viscous liquid which refuses to crystallise, and could not be obtained in a fit state for analysis. It dissolves somewhat sparingly in water, and behaves as an acid, its solution reddening litmus paper and neutralising alkalies.

Its lead salt is easily prepared by adding acetate of lead to its aqueous solution, when a white flocculent precipitate is produced. This is by no means completely insoluble, so that its bulk diminishes considerably on washing. The following results were obtained on submitting this compound to analysis:—

$$\begin{aligned} 0.1060 \text{ gave } 0.0925 \text{ PbSO}_4 &= 0.06319 \text{ Pb} = 59.6 \% \\ (\text{C}_7\text{H}_7)\text{POPb} \text{ requires } & . . . . . 60.0 \% \end{aligned}$$

These numbers seem to show that the lead salt is a derivative

of the oxide of monobenzyl phosphine  $\text{PH}_2(\text{C}_7\text{H}_7)\text{O}$ —in which both atoms of hydrogen are replaced by the metal.

The composition of the oxide was also established to a certain extent by the increase in weight which a sample of the pure phosphine experienced on spontaneous oxidation,—a rough experiment giving an increase of 14.1% instead of 12.9%, the calculated amount.

The authors regret that the small quantity of phosphine at their disposal prevented them from making further or more exact experiments. No oxide of a primary phosphine containing a single atom of oxygen has as yet been obtained (excluding the substance under discussion).

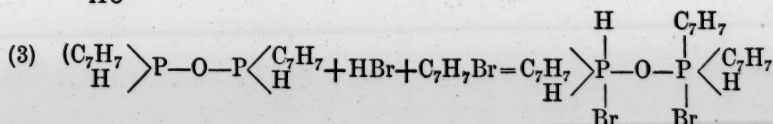
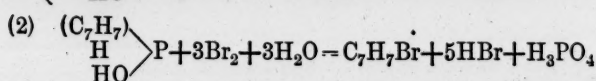
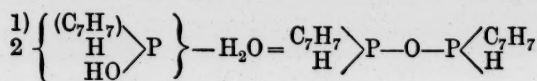
*Action of Bromine on Oxide of Monobenzyl Phosphine.*—When bromine is added to the syrupy oxide, it is rapidly decolorised, and the mixture grows hot, whilst hydrobromic acid is evolved, and the pungent odour of bromide of benzyl is noticed. If sufficient bromine has been added, a crop of crystals forms after some time, which dissolve both in water and ether, and may be obtained colourless by recrystallisation. These crystals, on analysis, gave the following results:—

0.221 gave 0.166  $\text{AgBr}$  = 0.07065  $\text{Br}$  = 31.9 %

0.384 gave  $\begin{cases} 0.6245 \text{ CO}_2 = 0.18735 \text{ C} = 48.9 \% \\ 0.1535 \text{ H}_2\text{O} = 0.01705 \text{ H} = 4.9 \% \end{cases}$

	Obtained.	Calculated for $(\text{C}_7\text{H}_7)_3\text{P}_2\text{Br}_2\text{H}_3\text{O}$
Bromine, . . . . .	31.9	31.1
Carbon, . . . . .	48.9	49.0
Hydrogen, . . . . .	4.9	4.7

The formula calculated from the analytical results is, as will be noticed, somewhat complex, and at a first glance may appear to be improbable. The following equations show, however, that such a body could conceivably be formed from the phosphine oxide—





Owing to the small quantity of the primary phosphine at our command, it was not possible to obtain sufficient of the brominated body for further experiments.

We also tried the action of alcoholic caustic potash and chloroform on the phosphine, to see whether, under these conditions, a phosphorised nitrile could be obtained but with negative results, the phosphine remaining unacted upon.

Our experiments with the primary phosphine are necessarily incomplete, owing to the very small quantity at our disposal, and the difficulty experienced in obtaining it in sufficient quantity.

*Dibenzyl Phosphine*.—This body is readily purified from the crude product described on p. 68 by two or three recrystallisations from boiling alcohol. It crystallises from that liquid in colourless needles, which are quite unchanged by exposure to air. Dibenzyl phosphine is only sparingly soluble in cold alcohol, but it dissolves pretty readily in boiling alcohol. It is readily soluble in chloroform, and is also soluble both in iodide of ethyl and in bisulphide of carbon. It is insoluble both in ether and in water. Glacial acetic acid is its best solvent. It melts at  $205^{\circ}$  (uncorrected), and sublimes at a higher temperature, with considerable decomposition.

The following are the results of its analysis :—

*Carbon and Hydrogen*—

$$\cdot 1795 \text{ gave } \begin{cases} \cdot 1171 \text{ H}_2\text{O} = \cdot 013011 \text{ H} = 7\cdot 2 \% \\ \cdot 5158 \text{ CO}_2 = \cdot 14067 \text{ C} = 78\cdot 4 \% \end{cases}$$

*Phosphorus*—

$$\cdot 3645 \text{ gave } \cdot 1963 \text{ Mg}_2\text{P}_2\text{O}_7 = \cdot 05473 \text{ P} = 15\cdot 0 \%$$

	Obtained.	Calculated for (C <sub>7</sub> H <sub>7</sub> ) <sub>2</sub> PH
Carbon, . . . .	78·4	78·5
Hydrogen, . . . .	7·2	7·0
Phosphorus, . . . .	15·0	14·5

Hofmann could not obtain any salts of dibenzyl phosphine, and considered it to be devoid of alkaline properties. We have found,

however, that although a very inert body yet under certain conditions, it does combine with the hydracids to form compounds, which, however, are unstable.

Among other compounds it forms a very characteristic salt with bromine, and it also combines with chloride of platinum.

*Platinum Salt.*—On mixing alcoholic solutions of chloride of platinum and dibenzyl phosphine a light yellow crystalline powder is produced, but its composition varies considerably according to the conditions under which it is prepared, as the following analyses show :—

	(A)	(B)	(C)
Carbon, . . .	59.5	56.6	58.5
Hydrogen, . . .	5.8	5.3	5.9
Platinum, . . .	12.8	13.1	13.1

(A) Prepared by mixing cold alcoholic solutions of chloride of platinum and dibenzyl phosphine, and washing the product with alcohol until the washings were colourless.

(B) Prepared by mixing very dilute boiling solutions of the two bodies, and repeatedly boiling with alcohol. (The alcohol dissolved a colourless body.) This product was very crystalline, and of a full yellow colour.

(C) Prepared as (B), but washed with cold alcohol.

It is probable that the products are loose compounds of chloride of platinum and dibenzyl phosphine. The nearest formula for A is  $5\{(C_7H_7)_2HP\}, PtCl_4$ , which requires

Carbon, . . .	59.5
Hydrogen, . . .	5.3
Platinum, . . .	14.0

*Action of Hydrobromic Acid on Dibenzyl Phosphine.*—An aqueous solution of hydrobromic acid is without effect on dibenzyl phosphine, but if the latter is dissolved in glacial acetic acid, and the mixture then saturated with hydrobromic acid gas, a crystalline precipitate falls, which usually redissolves as the solution grows warm, and subsequently, when the latter is saturated and has grown cool again, is deposited in small colourless crystals, which, when examined with the microscope, are found to consist of small but perfect plates.

Two separate specimens—A and B—of the compound were prepared, each being washed with glacial acetic acid, and finally dried *in vacuo* over sulphuric acid. B was prepared more carefully, and washed more thoroughly than A. Determinations of bromine gave the following results :—

	A.		B.	Calculated for	
	(1)	(2)		(Bz <sub>7</sub> HP), HBr	2(Bz <sub>2</sub> HP), HBr
Bromine,	19.5	20.5	16.3	27.4	15.9

The compound is very unstable. It is decomposed by boiling its solution in acetic acid with water, by dissolving it in alcohol, and by an alcoholic solution of potash—dibenzyl phosphine resulting. In the two first cases the decomposition is gradual, in the third it is immediate.

*Dibenzyl Phosphine and Hydriodic Acid.*—On passing gaseous hydriodic acid into a solution of dibenzyl phosphine in glacial acetic acid, a crystalline precipitate is produced, which dissolves as the solution grows warm, and subsequently is again deposited in thin rectangular plates. The compound, after careful washing with glacial acetic acid, and drying *in vacuo*, yielded the following numbers :—

	Obtained.	Calculated.
		2(Bz <sub>2</sub> HP), HI
Iodine, .	$\left\{ \begin{array}{l} 21.0 \\ 21.5 \end{array} \right\}$	22.8

The compound, like the hydrobromate, is unstable, and is decomposed in a precisely similar manner.

*Action of Bromine on Dibenzyl Phosphine.*—On mixing solutions of the phosphine and bromine in glacial acetic acid, heat is developed, and a light orange crystalline precipitate is thrown down. The compound was prepared several times under varying conditions. For analysis it was thoroughly washed with glacial acetic acid, then dried *in vacuo*.

The following results were obtained :—

	A.	B.	C.	D.	E.	F.
Carbon, . . .		56.5	...	56.5	56.4	...
Hydrogen, . . .		5.6	...	5.0	5.3	...
Bromine, .	26.0	26.4	27.2	...	26.7	26.6

A, B, C, D, E, and F were separate preparations.

At first we naturally anticipated that an addition product had

been obtained, as no free hydrobromic acid was observed, but this supposition seemed to be negatived by the fact that the above analyses indicate that for every molecule of the phosphine only one atom of bromine had been added. This fact appeared to indicate that a substitution product had really been formed, but as we shall show presently the reactions of the brominated body do not support such a view of its constitution.

The subjoined numbers show that only a very slight difference would exist in the composition of these two bodies, and that the analytical results obtained by us would agree almost equally well with either.

	Obtained Mean Results.	Calculated for	
		Bz <sub>2</sub> HP.Br	Bz <sub>2</sub> BrP.
Carbon, . . . .	56.5	57.1	57.3
Hydrogen, . . . .	5.3	5.1	4.8
Bromine, . . . .	26.5	27.2	27.3

We therefore turned to the reactions of the brominated body for some clue to its constitution.

The compound, when warmed with an alcoholic solution of potash or soda, was at once decolorised and a colourless body resulted, which crystallised from alcohol in colourless needles. A specimen of this crystalline body, after several crystallisations from alcohol, gave the following numbers:—

Carbon, . . . .	76.5
Hydrogen, . . . .	6.7

When boiled with spirit it was also decolorised, a volatile body smelling like bromide of benzyl being disengaged, whilst colourless crystals separated from the solution. These after recrystallisation gave an analysis:—

Carbon, . . . .	76.8
Hydrogen, . . . .	6.6

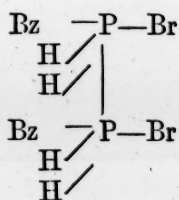
These two determinations proved that the brominated body decomposed when treated either with an alkali or with alcohol, and we suspected, from the appearance and properties of the crystalline body formed, that dibenzyl phosphine had been reproduced. The above numbers, however, do not agree with those required for dibenzyl phosphine, and we thought it possible that the brominated body had not been submitted for a sufficient length of time to the action of the decomposing agent.



Accordingly we prepared a new specimen of the brominated body with great care (specimen E). A sample of it was evaporated three times to dryness with alcoholic potash, then carefully recrystallised from alcohol. In appearance the product thus obtained exactly resembled the original dibenzyl phosphine. Its melting point ( $205^{\circ}$ ) was the same, and when treated with bromine it gave the characteristic yellow compound. On analysis numbers were obtained agreeing with those required for dibenzyl phosphine.

	Obtained.	Calculated for Bz <sub>2</sub> H P
Carbon, . . .	. 78.0	78.5
Hydrogen, . . .	. 7.1	7.0

We are of opinion that these experiments prove that the brominated body is a product of addition, and that the alkali simply removes the bromine; but as a compound of one atom bromine with a molecule of dibenzyl phosphine is not in harmony with the modern views of atomicity, the following structural formula may be written for the brominated compound:—



—indicating that the formula for dibenzyl phosphine itself should be doubled, and this would certainly account for its remarkable inertness, in which respect it differs from all secondary phosphines hitherto obtained. The compounds which it forms with the hydracids also favour this view of its constitution, for it will have been noticed that both the hydriodate and hydrobromate contain a single molecule of the hydracid for the double molecule of the phosphine.

*Action of Chlorine on Dibenzyl Phosphine.*—Chlorine was passed into a solution of the phosphine in glacial acetic acid, when a white body was first precipitated, but this dissolved up partly, and eventually a light yellow crystalline substance separated.

		Bz <sub>2</sub> HPCl
Chlorine, . . .	. 12.0	14.2

As the quantity of dibenzyl phosphine at our disposal was small, we could not repeat the experiment so as to obtain a new quantity of the chlorinated body for analysis, but it can scarcely be doubted that it is of the same nature as the brominated body, and that its formula is in all probability  $(C_7H_7)_4H_2P_2Cl_2$ .

*Examination of Bye Products.*—(A) *Crystalline Substance precipitated by Hydrochloric Acid from the Potash Solution used to extract the viscous mass containing the crude Dibenzyl Phosphine.*—This substance was precipitated in crystalline flocks on addition of hydrochloric acid to the potash solution. It was very sparingly soluble in cold water—rather more so in hot water, and crystallised from a boiling aqueous solution in indistinct leaflets. That the substance had acid properties was proved by the readiness with which it dissolved in potash solution and in caustic baryta. A slight residue was, however, left in both cases, and indeed an impurity appeared to be present extremely difficult to get rid of, as the following analyses show :—

	Obtained.			Calculated for $(C_7H_7)_2OHP\ O$
	(1)	(2)	(3)	
Carbon, .	59.69	66.3	67.06	68.3
Hydrogen, .	5.8	6.3	6.5	6.1
Phosphorus,	16.2	12.5		12.6

(1) Crude product washed with water.

(2) Precipitated from a solution of the crude product in baryta water by hydrochloric acid, and carefully washed (melting point  $183^\circ C$ ).

(3) Several times precipitated from baryta solution by hydrochloric acid, then recrystallised from alcohol and water (melting point  $185.5$ ).

*Barium Salt.*—Obtained by dissolving the crude product in baryta water and subsequent precipitation of the excess of baryta by a stream of carbonic anhydride. The salt crystallised from the highly concentrated solution in radiating tufts of crystals.

	Obtained.		Calculated for $\{(C_7H_7)_2PO_2\}Ba$
Barium, .	22.2	21.8	21.8
Water, .	24.0		Calculated for $\{(C_7H_7)_2PO_2\}_2Ba, 11H_2O$ 23.7

*Zinc Salt*.—Obtained as a white amorphous precipitate on adding acetate of zinc to a solution of the barium salt :—

	Obtained.	Calculated for $\{(C_7H_7)_2PO_2\}_2Zn$
Zinc, . . .	12.1	11.7

*Silver Salt*.—Obtained by adding a strong aqueous solution of nitrate of silver to a solution of the acid in alcohol, when the salt separated in thin colourless needles :—

	Obtained.	Calculated for $\{(C_7H_7)_2PO_2\}_2Ag$
Silver, . . .	30.1	30.6

Although the analyses of the acid itself are not very satisfactory, the composition of its salts shows pretty conclusively that it is dibenzyl phosphinic acid. This is also proved by its artificial production from dibenzyl phosphine, as we shall presently explain.

Dibenzyl phosphinic acid has the following properties :—

It is very sparingly soluble in water, but readily dissolves in hot alcohol. From a mixture of the two it crystallises in thin scales with mother-o'-pearl lustre. Its melting point is  $183^{\circ}$ – $186^{\circ}$  C. Its salts with the alkalies and alkaline earths are readily soluble, whilst those which it forms with lead, zinc, and silver are very sparingly soluble.

*Production of Dibenzyl Phosphinic Acid from Dibenzyl Phosphine*.—When dibenzyl phosphine is heated with caustic potash or soda it fuses and floats on the surface of the melted alkali. No violent action occurs, but on cooling the mixture and treating it with water, the greater portion dissolves, and acids then precipitate a flocky crystalline substance, which is dibenzyl phosphinic acid, as the following data prove :—

Melting point after two recrystallisations from a mixture of alcohol and water,  $186^{\circ}.5$  C.

	Obtained.	Calculated.
Carbon, . . .	67.1	68.3
Hydrogen, . . .	6.5	6.1
<i>Lead Salt</i> —		
Lead, . . .	30.5	29.7
<i>Barium Salt</i> —(dried at $110^{\circ}$ C.)		
Barium, . . .	22.0	21.8

We think it probable that the occurrence of the acid, as a by-product in the preparation of the secondary phosphine, is due to the digestion of the latter with strong potash solution.

*B. Solid Substance separating from the Aqueous Solution obtained by treating the contents of the tubes with Water.*—This separated out spontaneously from the aqueous solution after it had been concentrated, and consisted of a white powder stained brown by iodine.

On boiling with dilute spirit the crystals dissolved, and on cooling the solution deposited colourless very thin plates, which, when dried, had the lustre of mother-o'-pearl.

An analysis was not made, as the melting point, reactions, and appearance of the salts which the substance yielded proved sufficiently that it was dibenzyl phosphinic acid.

It may here be mentioned that this body is also produced when chloride of benzyl and phosphonium iodide are heated alone in sealed tubes, the product of action being afterwards treated with water.

*C. Viscous Substance remaining in the Alcoholic Mother Liquors after Dibenzyl Phosphine had crystallised out.*—This was squeezed out through a linen filter from the crude dibenzyl phosphine, and was obtained in considerable quantity. Various experiments were tried with it, but without getting any satisfactory results. At last it was found that boiling water dissolved a portion, and that in concentrating an oily liquid separated out, which partly solidified on cooling. By draining the crystals thus obtained on filter paper, and repeated recrystallising them from hot water, a product of constant melting point, viz.,  $104^{\circ}$  to  $105^{\circ}$ , was obtained.

On analysis the following numbers were obtained :—

	I.	II.	III.
Carbon, . . .	71.5	71.3	71.7
Hydrogen, . . .	6.86	6.96	6.78

The body had the following properties :—

(1) When heated most of it appeared to distil unchanged, but at the same time an odour similar to that of the primary phosphine was noticed.

(2) When its aqueous solution was mixed with caustic potash, It was precipitated unchanged.



(3) Chloride of platinum gave no sparingly soluble or crystalline compound.

(4) Bromine vapour liquefied it, and an additional quantity of bromine gave an oil which partly solidified.

(5) Aqueous solution of mercuric chloride gave an immediate flocculent white precipitate.

(6) Iodide of zinc gave an uncrystallisable oil.

(7) Iodide of cadmium gave with a dilute solution a white crystalline body soluble in boiling water, which crystallised as the solution cooled in minute square plates.

On analysing this compound, the following numbers were obtained :—

Iodine, . . . . .	31.5 %
Cadmium, . . . . .	12.3 %

If it be assumed that the percentage of iodine was correctly determined, the calculated percentage of cadmium amounts to 13.9, and the percentage of iodide of cadmium to 45.4.

On the assumption that the compound contains a single molecule of the latter, and two molecules of the phosphorised body, the molecular weight of the latter amounts to 220° C.

As the quantity of phosphorised body at our disposal was exceedingly small, we were not able to make any determinations of phosphorus.

Arguing, however, from the percentages of carbon and hydrogen, and from the molecular weight deduced as above, we find as the most probable formula for the body  $C_{13}H_{15}PO$ , as the following numbers show :—

	Obtained.	Calculated for $C_{13}H_{15}PO$ .
Molecular weight, . . . . .	220	216
% Carbon, . . . . .	71.5	71.5
% Hydrogen, . . . . .	6.9	6.9

Its reactions resemble those of a tertiary phosphine oxide ; for instance, its ready solubility in water, the fact that it distils almost unchanged, that its solution is precipitated by potash, and that it unites with iodide of cadmium.

We hesitate, however, to express any positive opinion with regard to its formula or constitution, as we have not sufficient data for a complete argument.

The body is possibly a tertiary phosphine oxide containing both aromatic and fatty radicals.

We have repeated some of the experiments described in this paper, and hope to be in a position to publish the results shortly.

5. A Criticism of the Theory of Subsidence as explaining the Origin of Coral Reefs. By H. B. Guppy, Esq., M.B., R.N. *Communicated by Dr H. MILL.* Published in the *Scottish Geographical Magazine.*

6. On the Compressibility of Water and of Different Solutions of Common Salt. By Prof. Tait.

(*Abstract.*)

Within the limits of the experiments, which were for  $t$  from  $0^{\circ}$  C. to  $15^{\circ}$  C., and for  $p$  from 1 to 3 tons-weight per square inch, it was found that the average compressibility of water per atmosphere may be *fairly* represented by

$$\frac{0.28}{(36 + p)(150 + t)},$$

a formula which, while very convenient for application in the hydrostatic equations, extends to the whole range of temperature and pressure ordinarily occurring in nature.

Some speculations, connected with Laplace's theory of Capillary Action and with the Kinetic Theory of Gases, are given as to the meaning of the 36 tons-weight per square inch which occurs in the formula.

Similar experiments made on solutions of common salt, of various strengths up to saturation, give analogous formulæ. As a *rough* indication of the results, it may be stated that at  $1^{\circ}$  C. the average compressibility per atmosphere for the first 150 atmospheres is somewhere about

$$\frac{0.002}{40 + s}$$

where  $s$  is the mass of salt dissolved in 100 of water.

Friday, January 6, 1888.

SIR WILLIAM THOMSON, F.R.S., President, in the Chair.

The following Communications were read:—

1. On a Practical Constant-Volume Air Thermometer.

By J. T. Bottomley, Esq.

In the fourth *Mémoire* of his celebrated *Relation des Expériences*, published in 1847, Regnault gives cogent reasons for preferring the air thermometer before any other as the instrument by means of which temperatures may be defined, and high temperatures determined. The thermodynamic researches of Sir William Thomson have furnished an absolute thermodynamic definition of temperatures; and the experimental researches of Dr Joule and Sir William Thomson have established the practical agreement of Regnault's air thermometers with the thermodynamic scale of temperatures. Lastly, the air thermometer is at present the only instrument, with the exception of a mercurial thermometer which has been compared with an air thermometer, by means of which temperatures higher than, say,  $150^{\circ}$  C. or  $200^{\circ}$  C. can be determined within  $3^{\circ}$  C. or  $4^{\circ}$  C.\*

In experimenting on the resistance of platinum and carbon filaments at high temperatures, in connection with a research on thermal radiation with which I have been engaged, I have used air thermometers of various forms; and I have recently been using a constant-volume air thermometer, which I first described to Professor Gray of University College, Bangor, just two years ago (January 1886), and partially constructed for him at that time. It is this instrument, greatly improved as to practical details, which I now desire to bring before the Royal Society.

The best known constant-volume air thermometer is that of Jolly of Vienna. It is a convenient instrument, and is fairly accurate for moderate temperatures; but for high temperatures a correction, which it is necessary to apply on account of expulsion of air from the heated part of the thermometer, becomes serious, at any rate

\* Mr H. L. Callendar has proposed to use the resistance of platinum for thermometric purposes; but in this case also the final standard of reference is the air thermometer.

with the dimensions commonly given to the instrument. It has also some other defects, among which may be mentioned difficulties as to the capillary surfaces of the mercury, want of flexibility or adaptability for various positions, and the proximity of the manometric column to the heated regions.

The modifications which I have made in the construction of the air thermometer have a threefold object, one part of which is to improve on the accuracy of the instrument, and reduce to the

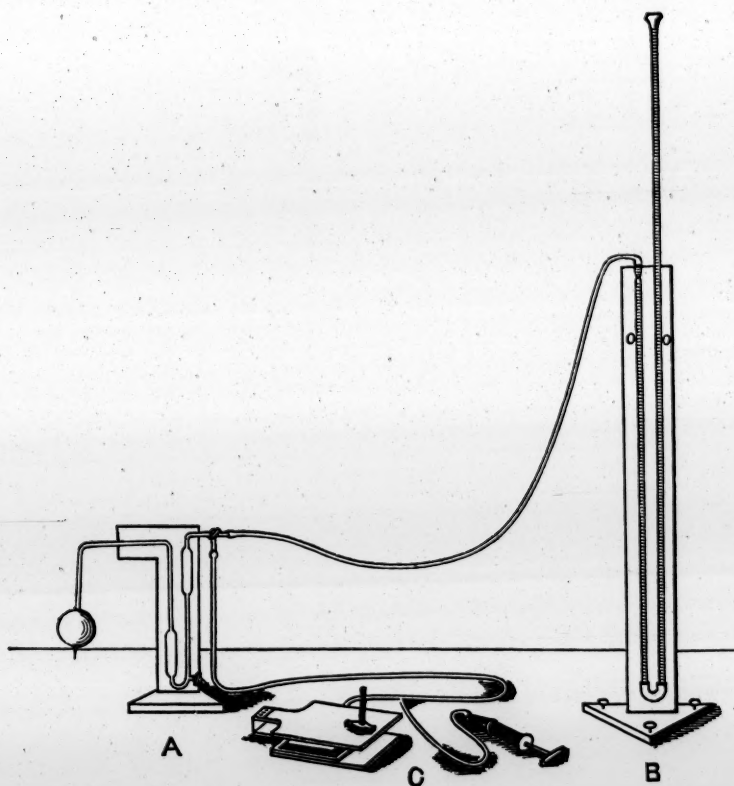


Fig. 1.

minimum that is practicable the correction above referred to for the air expelled by heat from the thermometer bulb or air reservoir. A second object is to increase the range of the instrument by giving it a form in which the hard Bohemian glass can be used in the construction of the part to be heated. The third object is to make that part of the thermometer which is to be heated, and which, in the use of the instrument, must be put in position with other pieces of experimental apparatus, of such a form as to be easily handled.



For all these objects I find it most convenient to construct separately the manometric columns, and the air reservoir with its volume indicator; connecting these two parts of the instrument only by flexible tubing. This arrangement necessitates an apparatus for regulating the pressure under which the air in the thermometer is maintained.

The complete instrument is shown in fig. 1. A is the air reservoir and volume indicator, B is the manometric gauge, and C is the pressure apparatus.

The air reservoir and volume indicator I shall call for brevity the volume gauge. It is made in two forms (figs. 2 and 3)—the form shown in fig. 2 for the lower, and the other for the higher temperatures. The bulb *a*, which is generally either globular or

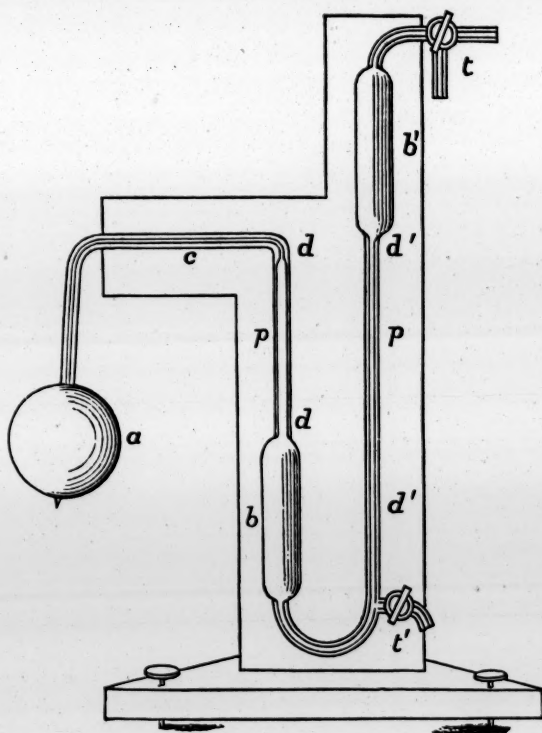


Fig. 2.

cylindrical, is connected by a very fine capillary tube *c* with a somewhat wider tube *d*. At *b*, *b'* there are two cylindrical bulbs of the same size. The tubes *dd* and *d'd'* are of precisely the same diameter, being cut from the same length of uniform glass tubing. The

diameter of this tube is about 1 mm. It is such tubing as is used for the fall tubes in a Sprengel pump.  $t$  and  $t'$  are two stop-cocks;  $t$  being a three-way stop-cock, connecting together the volume gauge, the manometric gauge, and the pressure pump; and  $t'$  is a stop-cock used for adjusting the quantity of liquid in the volume gauge.

The object of the two cylindrical reservoirs  $b$  and  $b'$  in the volume gauge is to give space into which the air in the bulb  $a$  may expand during heating, or in which a supply of the air may be kept during the cooling of the thermometer. The tube  $d$  is very small in capacity in comparison with the bulb; and were it not for these reser-

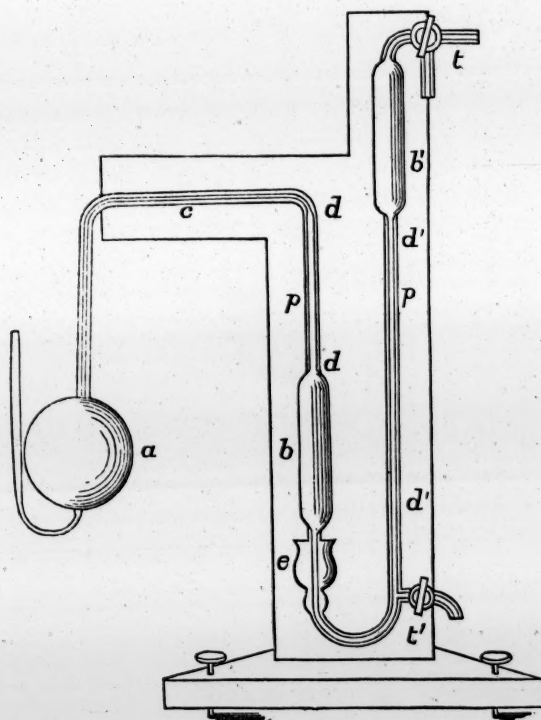


Fig. 3.

voirs, a very small change in temperature would cause the air to be driven out round the bend of the U, or the liquid in the bend to be drawn over into the bulb, unless the observer were incessantly on the watch to prevent this occurring by regulating the pressure.

The U of the volume gauge is filled so full of liquid that the equilibrium reading is taken at the points  $pp$  of the tubes  $d$  and  $d'$ ; and both in the selection of the tubes  $c$  and  $d$ , and in the glass-

blowing at the junction, as well as in the adjusting of the quantity of liquid in the bend, the endeavour is made to keep the volume of the air-space between the bulb and the point *p* as small as possible, consideration being given to the capillarity of the tube *d*. Either mercury or sulphuric acid may be used in the volume gauge. I prefer sulphuric acid on account of its smaller density. The greatness of the density of mercury, and the uncertainty of its capillary action, make its use very liable to produce serious errors in reading. But, on the other hand, in the case of sulphuric acid, the wetting of the tubes, which constitutes its advantageous quality so far as capillarity is concerned and gives regularity of capillary action which mercury never possesses, renders watchfulness necessary to keep the acid well clear of the fine tube *c*. If once the acid is allowed to enter that tube, it tends to make its way along it towards the bulb.

The manometric tube is simply a U tube capable of giving a difference of levels of from 100 to 150 centimetres of mercury, and wide enough to make capillarity very small and difference of capillarities in the two tubes negligible. With a tube giving a difference of levels of 150 centimetres, a temperature of about 550° C. may be reached, starting with air at normal density at common temperatures. The difference of levels may be read by means of a kathetometer, or, what is preferable, the tubes themselves may be graduated to millimetres. The tubes which I use are graduated from a zero line which is at the middle of the long branch of the U (see fig. 1). The longer tube is numbered upward, and the shorter downwards from the zero line, and the mercury is filled in so as to stand at the zero in both branches when there is no difference of pressure, and thus the sum of the readings of the two tubes is equal to the difference of pressures when any difference of pressure exists.

The pressure apparatus consists of a simple pressure syringe which forces air into a small air-bag of india-rubber fortified with canvas. The air-bag is placed between two boards, which are connected by a leather hinge, and pressed together by means of a nut which works on a wooden screw. The air-bag is also connected by means of a T tube with the three-way stop-cock *t'*; and, by means of this stop-cock, presses both on the liquid in the volume gauge, and on the shorter column of the manometer. The india-rubber tubes

used for these connections require to be strengthened with canvas to resist the pressure.

The form of volume gauge shown in fig. 3 is designed for use at very high temperatures. It is made in two parts, which are connected together at the cup *e* (shown enlarged, fig. 4). The bulb and tubes *c*, *d*, and *b*, are made of hard Bohemian glass; the remaining part of the gauge is of German glass or English flint glass. The stopper of the cup *e* is made to fit the throat of the cup closely, and

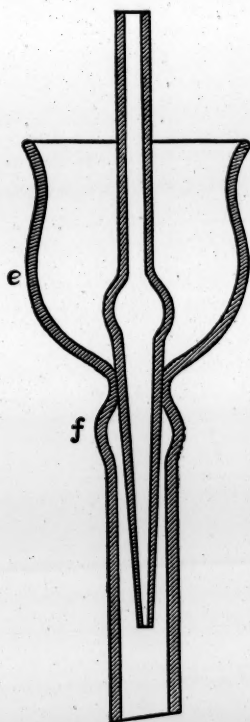


Fig. 4.

just below the throat an enlargement *f* is blown out, through which the elongated point of the stopper passes. The stopper is fastened air-tight into the cup with German "Siegelwachs"; and the object of the enlargement is to furnish a cushion of air which prevents the liquid of the volume gauge from coming in contact with the cement. The making of this joint is a little troublesome, and it requires to be protected against radiation from the hot source. There are various stoppers and joints well known, which prevent leakage inwards from without; but it is much more difficult to find an efficacious stopper which will act against pressure from within outwards.

The thermometer bulb is filled with perfectly pure dry air, and it is desirable to have the bulb filled with such a quantity of air that the pressure is approximately that of a normal atmosphere when the temperature is freezing. For, if the quantity of air be considerably greater than corresponds with this condition, there is a loss of range in the instrument; whereas, if there be but a small quantity of air, there is a tendency for the liquid of the volume gauge to be drawn over into the bulb when the temperature of the room comes down (as in winter it may) to about the freezing point, unless the instrument be left with the three-way stop-cock closed and the air under diminished pressure. For special circumstances the quantity of air may be made to suit the conditions; for, as Regnault has



shown, the results obtained from the instrument are but very slightly affected by the initial pressure of the air, and this with very wide limits; and by commencing at common temperatures with air of small density, very low pressure, the upper limit of the range may be extended without increasing the length of the manometric tubes.

The filling I accomplish in the following way:—The proper quantity of liquid is first introduced into the volume gauge, and the stop-cock *t* helps in introducing the liquid and in adjusting the quantity. For this and the subsequent operations I use a really good Bunsen water aspirator, with a Woulfe's two-necked bottle interposed between the aspirator and the work, and a good length of small bore non-collapsible india-rubber tubing. With the india-rubber tubing the apparatus to be exhausted can be turned into any required position while the exhaustion is being carried on, and air-bubbles can be got rid of with ease.

When the volume gauge has been supplied with liquid, I connect the three-way stop-cock *t'* to the aspirator, and draw the whole of the liquid up into the bulb *b'* and the tube leading up to the stop-cock itself. The size of the bulbs and of the tubes is, as has been explained, such that when this has been done the bulb *b* is empty as well as the tubes on the left-hand side of the gauge almost down to the bend. The three-way stop-cock is then closed, and the aspirator disconnected.

I now, with the help of a temporary three-way stop-cock, connect together the tail piece of the bulb shown in fig. 3, the aspirator, and a train of drying and purifying tubes (sulphuric acid and caustic potash). The arrangement is such that, on turning the tap of the three-way stop-cock into position No. 1, the aspirator draws the air out of the bulb; while, on turning it into position No. 2, air flows into the bulb passing through the drying tubes. The bulb is emptied and refilled many times; and during this process the bulb and all the tubes are heated with a Bunsen flame very nearly to the melting point of the glass.\* When it is perfectly certain that there is nothing but pure dry air in the bulb and tubes,

\* By this process every trace of moisture and condensed air is driven up from the walls of the tube; and, the bulb being filled with perfectly dry air, it seems certain, from the experiments of Bunsen and from experiments which

these are allowed to cool with free passage to the atmosphere through the drying tubes. The bulb is then surrounded with broken ice, and the three-way stop-cock  $t'$  is opened. The liquid of the volume gauge now finds its level; and, noting the barometer roughly (merely to know approximately the pressure), I seal the tail-piece at the extremity. The bulb now contains about the quantity of air required, and it is now only necessary to remove the tail-piece. For this purpose the ice is taken away, and the liquid of the gauge is once more drawn back to a considerable extent, thus making a partial vacuum to avoid blowing out of the air during sealing. The blowpipe flame can then be applied, and the sealing finished off as in fig. 2. Finally, the manometer and pressure apparatus are connected to the volume gauge, and the constant of the instrument is obtained by determining the pressure required, including the barometric pressure, to bring the liquid of the volume-gauge into the marked position, first at the temperature of melting ice, and then at the temperature of steam at normal pressure. When reading the standard barometer, I also, in accordance with a most convenient suggestion by Professor Quincke, read at the same time my standard aneroid; and this for most purposes, with occasional comparison with the standard, is amply sufficient to give the barometric variations. As in the case of the mercurial thermometer, so also in the air thermometer there is sure to be a secular contraction of the bulb; and, with the large bulbs used for the air thermometer it is quite possible that the redetermination of the constant of the air thermometer from time to time may be necessary.

Convenient formulæ for calculating temperatures from the indications of the air-thermometer are easily obtained. Such formulæ were given by Jolly (*Jubilband von Poggendorf's Annalen*), who also made fresh determinations of the expansion of air and other gases. Some of these formulæ are quoted in the *Leitfaden der Praktischen Physik* of Kohlrausch; but curiously enough there is nothing said in the description of the air-thermometer by Kohl-

I have myself carried out, that there is no subsequent perceptible condensation of air at the surface of the glass, such as has sometimes been supposed to vitiate the readings of the air thermometer. Air only condenses on the surface of the glass when there is moisture present—at any rate in such quantity as would be perceptible in a case like the present.

rausch, as to determination of the boiling point, the "ice point" merely being determined. An experimental determination of each point is, however, absolutely essential.

*Addition, June 5, 1888.*

Shortly after the reading of the foregoing paper, I commenced to use the coal-gas oxygen blowpipe—employing Fletcher's oxygen blowpipe and oxygen supplied in steel cylinders by the Scotch and Irish Oxygen Company (Brin Process). For convenience these cylinders, with the automatic apparatus supplied by the company for reducing the pressure of the gas, leave nothing to be desired; and the use of the oxygen blowpipe makes easy and simple many operations which were formerly all but impossible. In particular, the working of Bohemian tubing becomes, without the slightest exaggeration, as easy as that of common flint or soft German glass; and in addition it is a perfectly simple matter to make a junction between flint glass and Bohemian glass tubing (Bohemian glass does not join well with soft German tubing). Another great advantage in the use of oxygen with the Bohemian glass is, that the glass does not become porcelainised when worked with this flame, as it does when worked with the ordinary flame.

With this new power to assist I have now abandoned completely the form of gauge shown in fig. 3, and instead I am using a gauge in which the main part is made of flint glass (stop-cocks of Bohemian glass cannot, so far as I know, be procured), but in which the air bulb *a* and capillary tube *c* are made of Bohemian glass, and the two glasses joined together a little below the bend at the top of the tube *dd*. I have not yet been able to obtain from any of the first-class makers of Bohemian tube a supply of fine capillary tubes, but this I make for myself by fusing up a piece of thick wide Bohemian tubing and drawing it down.

2. On the Roots of  $\varepsilon^2 = -1$ . By Gustav Plarr, Docteur ès-Sciences Math. *Communicated by Prof. TAIT.*

The imaginaries of Algebra have done good service during the process of discovery of the principles of quaternions. Now that those principles have been founded on the basis of operations on real lines, we must no more admit  $\sqrt{-1}$  as the equivalent of a

unit-vector. Vectors, and unit-vectors, by their very definition, represent real lines, each of given direction and length, and a real quantity cannot be represented by an imaginary one.

We define the symbol  $U\rho$  to be the *operator* by which we construct the length  $T\rho$ , so as to give to that length the direction belonging to  $\rho$ . In expressing the vector  $\rho$  by the symbolical product

$$\rho = U\rho \times T\rho,$$

and in attributing reality to  $\rho$ , we are constrained to attribute reality to  $U\rho$  also.

We may put the expression of  $\rho$  under the form

$$\rho = (U\rho \times l) \times \left(\frac{T\rho}{l}\right),$$

where  $l$  is supposed to represent the unit line: thus  $(U\rho \times l)$ , or simply  $U\rho$ , represents a vector of unit length: hence the denomination of *unit-vector* given to  $U\rho$ .

For the proposed equation,  $\varepsilon^2 = -1$ , we remark first that the symbolic square of the unit-vector  $\varepsilon$  is not, properly speaking, a square in the algebraic sense, and that  $\varepsilon^2$  stands for the symbol  $\varepsilon\varepsilon$  which may be looked upon as a collocation arrived at in the process of the symbolic multiplication of two vectors, which contain the element  $\varepsilon$ . We must therefore refrain from thinking that the element  $\varepsilon$  can be extracted from the symbol  $\varepsilon\varepsilon$ , by simply applying to it an algebraical extraction of the square root.

There exists a more extensive class of cases, in which we must avoid the introduction of the symbol  $\sqrt{-1}$  in the place of unit-vectors, and for this end we shall propose the adoption of a certain principle which has its analogue in the principle of the inconvertibility of the order of the factors in a vector product.

We have, namely, for the versor

$$q = \cos u + \varepsilon \sin u,$$

the result

$$q^{\frac{n}{m}} = \cos w + \varepsilon \sin w,$$

where the fraction  $\frac{n}{m}$  is supposed to be reduced to its lowest terms, and

$$w = \frac{n}{m}u + \frac{2\pi}{m}N,$$



the integer  $N$  being susceptible of taking the  $m$  values

$$0, 1, 2, \dots, m-1.$$

When the angle  $u$  takes the value of a right angle, say

$$u = \frac{1}{2}\pi,$$

then we get

$$(q) = \varepsilon,$$

so that  $\varepsilon$  becomes the equivalent of what is called a quadrantal versor. In this case we have

$$\varepsilon^{\frac{n}{m}} = \cos w_1 + \varepsilon \sin w_1,$$

$$w_1 = \frac{1}{2}\pi \frac{n}{m} + \frac{2\pi}{m}N,$$

$N$  again taking the  $m$  values

$$0, 1, 2, \dots, (m-1).$$

The second member of this expression of  $\varepsilon^{\frac{n}{m}}$  represents  $m$  versors differing from each other, but they all are real quantities (real as opposed to imaginary of the form  $A + B\sqrt{-1}$ ), their axis being the unit-vector  $\varepsilon$ .

The first member, that is the expression of the  $\frac{n^{\text{th}}}{m}$  power of  $\varepsilon$ , may be considered principally under two forms, and for stating them we will suppose the case:

$$\frac{n}{m} = \frac{2n'}{2m'+1}.$$

The power  $\frac{2n'}{2m'+1}$  of  $\varepsilon$  may be calculated:

I. Either under the form

$$\left(\varepsilon^{\frac{1}{2m'+1}}\right)^{2n'},$$

II. or under the form

$$\left(\varepsilon^{2n'}\right)^{\frac{1}{2m'+1}}.$$

In this second case, applying  $\varepsilon^{2n'} = (\varepsilon^2)^{n'} = (-1)^{n'}$ , the result will be

$$(-1)^{\frac{n'}{2m'+1}}.$$

This is the expression of the roots of the scalar equation:

$$x^{2m'+1} - (-1)^{n'} = 0.$$

These roots are evidently, all of them, scalars, of the form  $A + B\sqrt{-1}$ , admitting  $\sqrt{-1}$  to be a scalar, in consequence of what is admitted in the theory of biquaternions.

We cannot, therefore, accept the expression II. for the representation of the *real versors*, depending on  $\varepsilon$ , which form the second members of the expression of the  $\frac{2n'}{2m'+1}$  power of  $\varepsilon$ , and we are consequently led to admit the form I. as being the true expression of that power of  $\varepsilon$ .

Generalising this result, we adopt the *principle* in virtue of which  $\varepsilon^{\frac{n}{m}}$  must be represented by  $\left(\varepsilon^{\frac{1}{m}}\right)^n$ , and not by  $(\varepsilon^n)^{\frac{1}{m}}$ .

We may apply this principle even to the case  $m=2$ ,  $n=2$ , when we get

$$(\varepsilon^{\frac{1}{2}})^2 = \varepsilon,$$

and we have to reject  $(\varepsilon^2)^{\frac{1}{2}}$

Should  $\frac{n}{m}$  converge towards an incommensurable value  $t$  (like that of a surd, &c.), we may still apply the principle by forbidding the separation of  $t$  into  $2 \times \frac{t}{2}$ , when  $\varepsilon^t$  would not be admitted equal to  $(\varepsilon^{\frac{t}{2}})^2$ ; but we might write  $(\varepsilon^{\frac{t}{2}})^2 = \varepsilon^t$ .

As to the point of view under which the symbolic square  $(U\rho)^2$  of a unit-vector  $U\rho$  may be looked upon, I would refer the reader to my paper printed in the volume xxvii. part ii. pages 175 to 202, of the *Trans. Roy. Soc. Edin.*, and particularly to the view taken there of treating *ii*, *ij*, *ik*, *ji*, &c., &c., each as a symbol by itself, to be determined by the condition that the products of two vectors should remain unaltered, whatever the system of the directions may be by which the components of the vector factors are reckoned.

### 3. On Vanishing Aggregates of Determinants. By Thomas Muir, LL.D.

1. In a paper\* communicated to the Berlin Academy on 27th July 1882, Kronecker pointed out that certain sets of minors of

\* Kronecker L., "Die Subdeterminanten symmetrischer Systeme," *Sitzungsb. d. k. Akad. d. Wiss.*, 1882, pp. 821-824.

any axisymmetric determinant are connected by a linear relation, or, as I have tried to put it more definitely in the present title, that certain aggregates of minors are equal to zero. This discovery has attracted considerable attention in Germany, as the list of papers herewith given suffices to show.\*

The object of the present communication is, in the first place, to point out how much the subject gains in simplicity and clearness, if we consider such identities altogether apart from axisymmetric determinants; and, in the second place, to direct attention to a new class of identities which have a similar special application.

2. Let us take then any general determinant of the  $n^{\text{th}}$  order, but for shortness' sake let it be written of the 5<sup>th</sup> order, viz.,

$$| a_1 b_2 c_3 d_4 e_5 |.$$

We have clearly at the outset the vanishing aggregate

$$\begin{aligned} & \begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5 \\ c_1 & c_2 & c_3 & c_4 & c_5 \\ d_1 & d_2 & d_3 & d_4 & d_5 \\ e_1 & e_2 & e_3 & e_4 & e_5 \end{vmatrix} - e_1 \begin{vmatrix} a_2 & a_3 & a_4 & a_5 \\ b_2 & b_3 & b_4 & b_5 \\ c_2 & c_3 & c_4 & c_5 \\ d_2 & d_3 & d_4 & d_5 \end{vmatrix} + e_2 \begin{vmatrix} a_1 & a_3 & a_4 & a_5 \\ b_1 & b_3 & b_4 & b_5 \\ c_1 & c_3 & c_4 & c_5 \\ d_1 & d_3 & d_4 & d_5 \end{vmatrix} \\ & - e_3 \begin{vmatrix} a_1 & a_2 & a_4 & a_5 \\ b_1 & b_2 & b_4 & b_5 \\ c_1 & c_2 & c_4 & c_5 \\ d_1 & d_2 & d_4 & d_5 \end{vmatrix} + e_4 \begin{vmatrix} a_1 & a_2 & a_3 & a_5 \\ b_1 & b_2 & b_3 & b_5 \\ c_1 & c_2 & c_3 & c_5 \\ d_1 & d_2 & d_3 & d_5 \end{vmatrix} - e_5 \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 & c_4 \\ d_1 & d_2 & d_3 & d_4 \end{vmatrix}. \end{aligned}$$

If we complete the first column of each of the last five determinants by inserting the elements A, B, C, D, the result is still a vanishing aggregate: and if the last row of the second determinant be completed by inserting the elements  $\alpha, \beta, \gamma, \delta$ , and if, at the same time, one of these elements be inserted in the place (5,2) of the remaining determinants, no alteration is even then made in the value of the aggregate: that is to say, we have the identity

\* Runge, C. "Die linearen Relationen zwischen den verschiedenen Subdeterminanten symmetrischer Systeme," *Crelle's Journ.*, xciii. pp. 319-327.

Mehmke, R. "Bemerkung über die Subdeterminanten symmetrischer Systeme," *Math. Annalen*, xxvi. pp. 209, 210.

Schendel, L. "Der Kronecker'sche Subdeterminantensatz," *Zeitsch. f. Math. u. Phys.*, xxxii. pp. 119, 120.

$$\begin{aligned}
 0 = & \begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5 \\ c_1 & c_2 & c_3 & c_4 & c_5 \\ d_1 & d_2 & d_3 & d_4 & d_5 \\ e_1 & e_2 & e_3 & e_4 & e_5 \end{vmatrix} - \begin{vmatrix} A & a_2 & a_3 & a_4 & a_5 \\ B & b_2 & b_3 & b_4 & b_5 \\ C & c_2 & c_3 & c_4 & c_5 \\ D & d_2 & d_3 & d_4 & d_5 \\ e_1 & a & \beta & \gamma & \delta \end{vmatrix} + \begin{vmatrix} A & a_1 & a_3 & a_4 & a_5 \\ B & b_1 & b_3 & b_4 & b_5 \\ C & c_1 & c_3 & c_4 & c_5 \\ D & d_1 & d_3 & d_4 & d_5 \\ e_2 & a & & & \end{vmatrix} \\
 & - \begin{vmatrix} A & a_1 & a_2 & a_4 & a_5 \\ B & b_1 & b_2 & b_4 & b_5 \\ C & c_1 & c_2 & c_4 & c_5 \\ D & d_1 & d_2 & d_4 & d_5 \\ e_3 & \beta & & & \end{vmatrix} + \begin{vmatrix} A & a_1 & a_2 & a_3 & a_5 \\ B & b_1 & b_2 & b_3 & b_5 \\ C & c_1 & c_2 & c_3 & c_5 \\ D & d_1 & d_2 & d_3 & d_5 \\ e_4 & \gamma & & & \end{vmatrix} - \begin{vmatrix} A & a_1 & a_2 & a_3 & a_4 \\ B & b_1 & b_2 & b_3 & b_4 \\ C & c_1 & c_2 & c_3 & c_4 \\ D & d_1 & d_2 & d_3 & d_4 \\ e_5 & \delta & & & \end{vmatrix}.
 \end{aligned}$$

Continuing this last process, viz., completing the 5<sup>th</sup> row of the third determinant, by inserting the elements X, Y, Z, and at the same time inserting one of these new elements in the place (5,3) of the remaining determinants, and so on, we finally arrive at the identity

$$\begin{aligned}
 & \begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5 \\ c_1 & c_2 & c_3 & c_4 & c_5 \\ d_1 & d_2 & d_3 & d_4 & d_5 \\ e_1 & e_2 & e_3 & e_4 & e_5 \end{vmatrix} - \begin{vmatrix} A & a_2 & a_3 & a_4 & a_5 \\ B & b_2 & b_3 & b_4 & b_5 \\ C & c_2 & c_3 & c_4 & c_5 \\ D & d_2 & d_3 & d_4 & d_5 \\ e_1 & a & \beta & \gamma & \delta \end{vmatrix} + \begin{vmatrix} A & a_1 & a_3 & a_4 & a_5 \\ B & b_1 & b_3 & b_4 & b_5 \\ C & c_1 & c_3 & c_4 & c_5 \\ D & d_1 & d_3 & d_4 & d_5 \\ e_1 & a & X & Y & Z \end{vmatrix} \\
 & - \begin{vmatrix} A & a_1 & a_2 & a_4 & a_5 \\ B & b_1 & b_2 & b_4 & b_5 \\ C & c_1 & c_2 & c_4 & c_5 \\ D & d_1 & d_2 & d_4 & d_5 \\ e_3 & \beta & X & p & q \end{vmatrix} + \begin{vmatrix} A & a_1 & a_2 & a_3 & a_5 \\ B & b_1 & b_2 & b_3 & b_5 \\ C & c_1 & c_2 & c_3 & c_5 \\ D & d_1 & d_2 & d_3 & d_5 \\ e_4 & \gamma & Y & p & \omega \end{vmatrix} - \begin{vmatrix} A & a_1 & a_2 & a_3 & a_4 \\ B & b_1 & b_2 & b_3 & b_4 \\ C & c_1 & c_2 & c_3 & c_4 \\ D & d_1 & d_2 & d_3 & d_4 \\ e_5 & \delta & Z & q & \omega \end{vmatrix} = 0,
 \end{aligned}$$

involving  $5^2 + 2 \cdot 4 + 3 + 2 + 1$  elements, or in general  $\frac{1}{2}(n+1)(3n-2)$ .

3. The connection of this with Kronecker's theorem is easily made apparent. Take a determinant with umbral elements, and for the sake of variety, let it now be of the 4<sup>th</sup> order, viz.—

$$\begin{vmatrix} 1,1 & 1,2 & 1,3 & 1,4 \\ 2,1 & 2,2 & 2,3 & 2,4 \\ 3,1 & 3,2 & 3,3 & 3,4 \\ 4,1 & 4,2 & 4,3 & 4,4 \end{vmatrix},$$

and let the new elements inserted during the formation of the identity be



$$\begin{array}{ccc}
 1,4 & 2,4 & 3,4 \\
 5,6 & 5,7 & 5,8 \\
 6,7 & 6,8 & \\
 7,8 & & 
 \end{array}$$

so that the identity is

$$\begin{vmatrix} 15 & 16 & 17 & 18 \\ 25 & 26 & 27 & 28 \\ 35 & 36 & 37 & 38 \\ 45 & 46 & 47 & 48 \end{vmatrix} - \begin{vmatrix} 14 & 16 & 17 & 18 \\ 24 & 26 & 27 & 28 \\ 34 & 36 & 37 & 38 \\ 45 & 56 & 57 & 58 \end{vmatrix} + \begin{vmatrix} 14 & 15 & 17 & 18 \\ 24 & 25 & 27 & 28 \\ 34 & 35 & 37 & 38 \\ 46 & 56 & 67 & 68 \end{vmatrix} \\
 - \begin{vmatrix} 14 & 15 & 16 & 18 \\ 24 & 25 & 26 & 28 \\ 34 & 35 & 36 & 38 \\ 47 & 57 & 67 & 78 \end{vmatrix} + \begin{vmatrix} 14 & 15 & 16 & 17 \\ 24 & 25 & 26 & 27 \\ 34 & 35 & 36 & 37 \\ 48 & 58 & 68 & 78 \end{vmatrix} = 0.$$

A glance at this suffices to show that the second determinant here would be a minor of any determinant of the 8<sup>th</sup> order, in which the elements 54 and 45 were identical: that the third determinant would likewise be a minor of the same determinant, if

$$46 = 64 \text{ and } 56 = 65;$$

also the fourth determinant, if

$$47 = 74, \quad 57 = 75, \quad 67 = 76;$$

and the fifth determinant, if

$$48 = 84, \quad 58 = 85, \quad 68 = 86, \quad 78 = 87.$$

Now in the axisymmetric determinant

$$11, 22, 33, \dots, 88 \mid_{rs=sr}$$

all these conditions hold. Consequently the above identity is an identity connecting five of the minors of

$$\mid 11, 22, 33, \dots, 88 \mid_{rs=sr};$$

and this is Kronecker's theorem.

4. It has been said that the number of elements occurring in the identity is  $\frac{1}{2}(n+1)(3n-2)$ ,  $n$  being the order of the determinants involved. When, therefore, the identity is given in connection with an axisymmetric determinant of the  $2n^{\text{th}}$  order, which, as we know, has  $n(2n+1)$  distinct elements, it is suggested to inquire which elements of the latter do not occur. Their number evidently is

$$\begin{aligned}
 & n(2n+1) - \frac{1}{2}(n+1)(3n-2) \\
 & \text{i.e. } \frac{1}{2}n^2 + \frac{1}{2}n + 1
 \end{aligned}$$

Now among these the  $2n$  elements of the axis of symmetry (or main diagonal) of the determinant must be included, as from the law of formation of the determinants of the identity their presence in the determinants is impossible. Consequently the number of elements outside the main diagonal of an axisymmetric determinant of the  $2n^{\text{th}}$  order, which are not involved in Kronecker's identity, is

$$\begin{aligned} & \frac{1}{2}n^2 - \frac{3}{2}n + 1, \\ & \text{i.e. } \frac{1}{2}(n-2)(n-1), \\ & \text{i.e. } 1 + 2 + 3 + \dots + (n-2). \end{aligned}$$

For example, when  $2n=6$ , the number not involved is 1 only: thus, in the case of the axisymmetric determinant of the  $6^{\text{th}}$  order

$$\begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 & a_6 \\ a_2 & b_2 & b_3 & b_4 & b_5 & b_6 \\ a_3 & b_3 & c_3 & c_4 & c_5 & c_6 \\ a_4 & b_4 & c_4 & d_4 & d_5 & d_6 \\ a_5 & b_5 & c_5 & d_5 & e_5 & e_6 \\ a_6 & b_6 & c_6 & d_6 & e_6 & f_6 \end{vmatrix} \quad \text{or D, say,}$$

the typical identity is

$$\begin{vmatrix} a_4 & a_5 & a_6 \\ b_4 & b_5 & b_6 \\ c_4 & c_5 & c_6 \end{vmatrix} - \begin{vmatrix} a_3 & a_5 & a_6 \\ b_3 & b_5 & b_6 \\ c_4 & d_5 & d_6 \end{vmatrix} + \begin{vmatrix} a_3 & a_4 & a_6 \\ b_3 & b_4 & b_6 \\ c_5 & d_5 & e_6 \end{vmatrix} - \begin{vmatrix} a_3 & a_4 & a_5 \\ b_3 & b_4 & b_5 \\ c_6 & d_6 & e_6 \end{vmatrix} = 0,$$

which involves all the elements outside the main diagonal, except  $a_2$ .

5. Denoting by

$$\begin{vmatrix} 1 & 2 & 3 \\ 4 & 5 & 6 \end{vmatrix}$$

that minor of D whose elements belong to the  $1^{\text{st}}$ ,  $2^{\text{nd}}$  and  $3^{\text{rd}}$  rows, and  $4^{\text{th}}$ ,  $5^{\text{th}}$  and  $6^{\text{th}}$  columns of D, we may write this identity in the more convenient form

$$\begin{vmatrix} 1 & 2 & 3 \\ 4 & 5 & 6 \end{vmatrix} - \begin{vmatrix} 1 & 2 & 4 \\ 3 & 5 & 6 \end{vmatrix} + \begin{vmatrix} 1 & 2 & 5 \\ 3 & 4 & 6 \end{vmatrix} - \begin{vmatrix} 1 & 2 & 6 \\ 3 & 4 & 5 \end{vmatrix} = 0.$$

It is then easily seen that the exclusion of the main diagonal elements is accomplished by making the numbers of the rows (e.g. 1, 2, 5), different from the numbers of the columns (e.g. 3, 4, 6): and that the exclusion of the one other element  $a_2$ , which occurs in both the  $1^{\text{st}}$  and  $2^{\text{nd}}$  columns of D, is accomplished by always including the numbers 1 and 2 among the numbers of the rows,

and thereby preventing them occurring among the numbers of the columns.

Further, we readily conclude that as there are fifteen elements outside the main diagonal of  $D$ , there must be connected with  $D$  fifteen identities like the above, the four determinants of each identity being easily got as soon as the element to be omitted has been decided upon. The number of different determinants involved in the fifteen identities is the number of ways in which the numbers 1, 2, 3, 4, 5, 6, can be separated into two sets, and therefore, is  $\frac{1}{2}C_{6,3}$  i.e. 10. Of these fifteen identities connecting ten determinants, it will be found that only five are independent.

6. Further, each identity consists of  $4 \times 6$ , i.e. 24, terms separable into 12 pairs, the terms of each pair being equal in magnitude, and opposite in sign. Now it is a curious fact, that the twelve different terms are exactly the twelve terms of the Pfaffian got from  $D$ , by deleting the elements which are not found in the identity. For example, the twelve different terms of the typical identity given above, are exactly the twelve terms of the Pfaffian,

$$\begin{vmatrix} 0 & a_3 & a_4 & a_5 & a_6 \\ & b_3 & b_4 & b_5 & b_6 \\ & & c_4 & c_5 & c_6 \\ & & & d_5 & d_6 \\ & & & & e_6 \end{vmatrix};$$

so that, in fact, the identity may be put in the form

$$\begin{vmatrix} a_4 & a_5 & a_6 \\ b_4 & b_5 & b_6 \\ c_4 & c_5 & c_6 \end{vmatrix} - \begin{vmatrix} a_3 & a_5 & a_6 \\ b_3 & b_5 & b_6 \\ c_4 & d_5 & d_6 \end{vmatrix} + \begin{vmatrix} a_3 & a_4 & a_6 \\ b_3 & b_4 & b_6 \\ c_5 & d_5 & e_6 \end{vmatrix} - \begin{vmatrix} a_3 & a_4 & a_5 \\ b_3 & b_4 & b_5 \\ c_6 & d_6 & e_6 \end{vmatrix} \\ = \begin{vmatrix} a_3 & a_4 & a_5 & a_6 \\ b_3 & b_4 & b_5 & b_6 \\ c_4 & c_5 & c_6 \\ d_5 & d_6 \\ e_6 \end{vmatrix} - \begin{vmatrix} a_3 & a_4 & a_5 & a_6 \\ b_3 & b_4 & b_5 & b_6 \\ c_4 & c_5 & c_6 \\ d_5 & d_6 \\ e_6 \end{vmatrix}, \\ = 0.$$

Apparently this is equally true for all the higher orders. Thus, in the case of the axisymmetric determinant,

$$| 11, 22, 33, \dots, 88 |_{rs=2}$$

we have

$$\begin{vmatrix} 15 & 16 & 17 & 18 \\ 25 & 26 & 27 & 28 \\ 35 & 36 & 37 & 38 \\ 45 & 46 & 47 & 48 \end{vmatrix} - \begin{vmatrix} 14 & 16 & 17 & 18 \\ 24 & 26 & 27 & 28 \\ 34 & 36 & 37 & 38 \\ 45 & 56 & 57 & 58 \end{vmatrix} + \begin{vmatrix} 14 & 15 & 17 & 18 \\ 24 & 25 & 27 & 28 \\ 34 & 35 & 37 & 38 \\ 46 & 56 & 67 & 68 \end{vmatrix} \\
 - \begin{vmatrix} 14 & 15 & 16 & 18 \\ 24 & 25 & 26 & 28 \\ 34 & 35 & 36 & 38 \\ 47 & 57 & 67 & 78 \end{vmatrix} + \begin{vmatrix} 14 & 15 & 16 & 17 \\ 24 & 25 & 26 & 27 \\ 34 & 35 & 36 & 37 \\ 48 & 58 & 68 & 78 \end{vmatrix} \\
 = \begin{vmatrix} . & . & 14 & 15 & 16 & 17 & 18 \\ . & 24 & 25 & 26 & 27 & 28 \\ 34 & 35 & 36 & 37 & 38 \\ 45 & 46 & 47 & 48 \\ 56 & 57 & 58 \\ 67 & 68 \\ 78 \end{vmatrix} - \begin{vmatrix} . & . & 14 & 15 & 16 & 17 & 18 \\ . & 24 & 25 & 26 & 27 & 28 \\ 34 & 35 & 36 & 37 & 38 \\ 45 & 46 & 47 & 48 \\ 56 & 57 & 58 \\ 67 & 68 \\ 78 \end{vmatrix},$$

the zero-elements of the Pfaffian occupying as before the places of those elements of the axisymmetric determinant, which do not occur in the identity, and which, in this case, are three in number (*v.* § 4).

7. The next theorem in regard to vanishing aggregates of determinants lends itself readily to formal enunciation. It is as follows:—

*If any two determinants A and B of the n<sup>th</sup> order be taken, and from them two sets of determinants be formed, viz., first, a set of n determinants, each of which is in one row identical with A, and in the remaining rows with B, and secondly, a set of n determinants, each of which is in one column identical with A, and in the remaining columns with B, then the sum of the first set of determinants is equal to the sum of the second set.*

Let the two determinants A and B be

$$\begin{vmatrix} a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 \\ c_1 & c_2 & c_3 \end{vmatrix}, \quad \begin{vmatrix} a_4 & a_5 & a_6 \\ b_4 & b_5 & b_6 \\ c_4 & c_5 & c_6 \end{vmatrix};$$

then the first set of determinants derived from them is



$$\begin{vmatrix} a_1 & a_2 & a_3 \\ b_4 & b_5 & b_6 \\ c_4 & c_5 & c_6 \end{vmatrix} + \begin{vmatrix} a_4 & a_5 & a_6 \\ b_1 & b_2 & b_3 \\ c_4 & c_5 & c_6 \end{vmatrix} + \begin{vmatrix} a_4 & a_5 & a_6 \\ b_4 & b_5 & b_6 \\ c_1 & c_2 & c_3 \end{vmatrix},$$

and the second set

$$\begin{vmatrix} a_1 & a_5 & a_6 \\ b_1 & b_5 & b_6 \\ c_1 & c_5 & c_6 \end{vmatrix} + \begin{vmatrix} a_4 & a_2 & a_6 \\ b_4 & b_2 & b_6 \\ c_4 & c_2 & c_6 \end{vmatrix} + \begin{vmatrix} a_4 & a_5 & a_3 \\ b_4 & b_5 & b_3 \\ c_4 & c_5 & c_3 \end{vmatrix}.$$

Expressing each determinant of the first set in terms of the elements of a row and their complementary minors, viz., the first determinant in terms of the elements of the first row, the second determinant in terms of the elements of the second row, and so on, we obtain the  $n \times n$  terms

$$\begin{aligned} & a_1|b_5c_6| - a_2|b_4c_6| + a_3|b_4c_5|, \\ & -b_1|a_5c_6| + b_2|a_4c_6| - b_3|a_4c_5|, \\ & +c_1|a_5b_6| - c_2|a_4b_6| + c_3|a_4b_5|. \end{aligned}$$

But the sum of all the first terms of the expansions is expressible as a determinant of the third order, so also is the sum of all the second terms, and so on; the result being

$$|a_1b_5c_6| + |a_4b_2c_6| + |a_4b_5c_3|,$$

as was to be proved.

8. This new identity, it will be seen, depends for its existence on the possibility of a double application of a certain expansion-theorem; and as this theorem is but the simplest case of Laplace's expansion-theorem, we are prepared to find that the dependent identity likewise is widely generalisable, so as, in fact, to be co-extensive with the theorem of Laplace. The generalisation is as follows:—

*If any two determinants A and B of the  $n^{\text{th}}$  order be taken, and from them two sets of determinants be formed, viz., first, a set of  $n(n-1) \dots (n-r+1) \cdot 1 \cdot 2 \dots r$  determinants, each of which is in  $r$  rows identical with A, and in the remaining rows with B, and secondly, a set of the same number of determinants, each of which is in  $r$  columns identical with A, and in the remaining columns with B, then the sum of the first set of determinants is equal to the sum of the second set.*

Thus taking for A and B the determinants

$$|a_1 b_2 c_3 d_4|, |a_5 b_6 d_7 e_8|,$$

and forming from them the set of six determinants

$$\begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_5 & c_6 & c_7 & c_8 \\ d_5 & d_6 & d_7 & d_8 \end{vmatrix}, \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_5 & b_6 & b_7 & b_8 \\ c_1 & c_2 & c_3 & c_4 \\ d_5 & d_6 & d_7 & d_8 \end{vmatrix}, \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_5 & b_6 & b_7 & b_8 \\ c_5 & c_6 & c_7 & c_8 \\ d_1 & d_2 & d_3 & d_4 \end{vmatrix},$$

$$\begin{vmatrix} a_5 & a_6 & a_7 & a_8 \\ b_1 & b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 & c_4 \\ d_5 & d_6 & d_7 & d_8 \end{vmatrix}, \begin{vmatrix} a_5 & a_6 & a_7 & a_8 \\ b_1 & b_2 & b_3 & b_4 \\ c_5 & c_6 & c_7 & c_8 \\ d_1 & d_2 & d_3 & d_4 \end{vmatrix}, \begin{vmatrix} a_5 & a_6 & a_7 & a_8 \\ b_5 & b_6 & b_7 & b_8 \\ c_1 & c_2 & c_3 & c_4 \\ d_1 & d_2 & d_3 & d_4 \end{vmatrix},$$

by repeatedly choosing two rows from A and two from B, we then expand each of the six in terms of minors of the second order and their complementary minors, the minors being formed in every case from the rows originally taken from A. There will be six terms in each of the six expansions; so that if we write the second expansion under the first, the third under the second, and so on, we shall have a collection of thirty-six terms which may be viewed as arranged in six columns as well as in six rows. Expressing the sum of the terms in each column as a determinant, we find for the total

$$|a_1 b_2 c_7 d_8| + |a_1 b_6 c_3 d_8| + |a_1 b_6 c_7 d_4| \\ + |a_5 b_2 c_3 d_8| + |a_5 b_2 c_7 d_4| + |a_5 b_6 c_3 d_4|,$$

in accordance with the theorem.

9. If A and B be the alternants

$$|a^0 b^1 c^2 d^3|, |a^s b^{s+1} c^{s+2} d^{s+3}|$$

the identity becomes

$$(a^s b^s + a^s c^s + a^s d^s + b^s c^s + b^s d^s + c^s d^s) |a^0 b^1 c^2 d^3| \\ = |a^0 b^1 c^{s+2} d^{s+3}| + |a^0 b^{s+1} c^2 d^{s+3}| + |a^0 b^{s+1} c^{s+2} d^3| + |a^s b^1 c^2 d^{s+3}| \\ + |a^s b^1 c^{s+2} d^3| + |a^s b^{s+1} c^2 d^3|,$$

or

$$\Sigma a^s b^s = a(0, 1, s+2, s+3) + a(0, s+1, 2, s+3) + a(0, s+1, s+2, 3) \\ + a(s, 1, 2, s+2) + a(s, 1, s+2, 3) + a(s, s+1, 2, 3),$$

which we know on other grounds to be true.

10. Returning now to § 7, and taking the originating determinants A and B in the form

$$\begin{vmatrix} 41 & 42 & 43 \\ 51 & 52 & 53 \\ 61 & 62 & 63 \end{vmatrix}, \begin{vmatrix} 46 & 45 & 44 \\ 56 & 55 & 54 \\ 66 & 65 & 64 \end{vmatrix},$$

that is to say, with their elements in the umbral notation, we obtain the identity in the form

$$\begin{vmatrix} 41 & 45 & 44 \\ 51 & 55 & 54 \\ 61 & 65 & 64 \end{vmatrix} + \begin{vmatrix} 46 & 42 & 44 \\ 56 & 52 & 54 \\ 66 & 62 & 64 \end{vmatrix} + \begin{vmatrix} 46 & 45 & 43 \\ 56 & 55 & 53 \\ 66 & 65 & 63 \end{vmatrix} \\ = \begin{vmatrix} 41 & 42 & 43 \\ 56 & 55 & 54 \\ 66 & 65 & 64 \end{vmatrix} + \begin{vmatrix} 46 & 45 & 44 \\ 51 & 52 & 53 \\ 66 & 65 & 64 \end{vmatrix} + \begin{vmatrix} 46 & 45 & 44 \\ 56 & 55 & 54 \\ 61 & 62 & 63 \end{vmatrix}.$$

Now the first three determinants here are minors of any determinant of the 6<sup>th</sup> order, and the second three *would be* minors of a determinant of the 6<sup>th</sup> order if its elements were such that

$$\begin{aligned} &41, 42, 43, 51, 52, 53, 61, 62, 63 \\ &= 36, 35, 34, 26, 25, 24, 16, 15, 14, \end{aligned}$$

—in other words, were such that  $r, s = 7 - r, 7 - s$ . But this is exactly the definition of a centro-symmetric determinant. Consequently the above identity is an identity connecting six of the minors of the centro-symmetric determinant

$$| 11, 22, 33, \dots, 66 |_{rs=7-r, 7-s},$$

and thus we have in regard to such determinants a theorem quite co-ordinate with Kronecker's regarding axisymmetric determinants.

In the notation of § 5 it would stand as follows:—

If  $| 11, 22, \dots, 66 |$  be centro-symmetric, i.e. if its elements be such that in every case  $r, s = 7 - r, 7 - s$ , then

$$\begin{vmatrix} 4 & 5 & 6 \\ 4 & 5 & 1 \end{vmatrix} + \begin{vmatrix} 4 & 5 & 6 \\ 4 & 2 & 6 \end{vmatrix} + \begin{vmatrix} 4 & 5 & 6 \\ 3 & 5 & 6 \end{vmatrix} \\ = \begin{vmatrix} 4 & 5 & 1 \\ 4 & 5 & 6 \end{vmatrix} + \begin{vmatrix} 4 & 2 & 6 \\ 4 & 5 & 6 \end{vmatrix} + \begin{vmatrix} 3 & 5 & 6 \\ 4 & 5 & 6 \end{vmatrix}.$$

4. On a Simplified Proof of Maxwell's Theorem. By Professor Burnside. Communicated by Professor TAIT.

In the course of verifying some of the mathematical work in the third instalment of Professor Tait's paper on the Kinetic Theory, the following simplification of his proof of Maxwell's theorem occurred to me.

The number  $dN$  of pairs of particles, one from a set  $(P, h)$ , the other from a set  $(Q, k)$ , for which the velocities parallel to the axes lie between

$x$  and  $x + dx$ ,  $y$  and  $y + dy$ ,  $z$  and  $z + dz$  in the one case, and between

$$x' \text{ and } x' + dx', y' \text{ and } y' + dy', z' \text{ and } z' + dz' \text{ in the other,} \\ \propto e^{-h(x^2+y^2+z^2)-k(x'^2+y'^2+z'^2)} dx dy dz dx' dy' dz' \dots \dots \dots (1.)$$

Write

$$Px + Qx' = (P + Q)a \\ x - x' = a'$$

and similar substitutions for  $y, y'$  and  $z, z'$ .

Then

$$hx^2 + kx'^2 = (h + k)a^2 + \frac{Q^2h + P^2k}{(P + Q)^2}a'^2 + 2\frac{Qh - Pk}{P + Q}aa' \\ = aa^2 + ba'^2 + 2caa' \text{ say.}$$

$$\therefore dN \propto e^{-a(a^2+\beta^2+\gamma^2)-b(a'^2+\beta'^2+\gamma'^2)-2c(aa'+\beta\beta'+\gamma\gamma')} \\ \times da d\beta d\gamma da' d\beta' d\gamma'$$

Hence if

$$a^2 + \beta^2 + \gamma^2 = \bar{v}^2 \\ a'^2 + \beta'^2 + \gamma'^2 = \bar{v}_0'^2 \\ aa' + \beta\beta' + \gamma\gamma' = \bar{v}\bar{v}_0' \cos \theta,$$

the numbers of pairs of particles one from each system for which the velocity of the centre of inertia lies between  $\bar{v}$  and  $\bar{v} + d\bar{v}$ , the relative velocity between  $v_0$  and  $v_0 + dv_0$ , and the angle included by the directions of  $\bar{v}$  and  $v_0$  between  $\theta$  and  $\theta + d\theta$

$$\propto e^{-a\bar{v}^2 - b v_0'^2 - c\bar{v}v_0' \cos \theta} \bar{v}^2 v_0'^2 d\bar{v} dv_0' \sin \theta d\theta.$$

The energy exchanged between a P and a Q at impact is

$$\frac{2PQ}{P + Q} \times \text{product of components of } \bar{v} \text{ and } v_0 \text{ in the line of} \\ \text{centres at impact.}$$



If  $\gamma$  is the angle between the line of centres and the direction of  $v_0$ , and  $\phi$  the angle between the plane parallel to these directions and the plane parallel to the directions of  $\bar{v}$  and  $v_0$ , the above quantity is

$$\frac{2PQ}{P+Q} \bar{v} v_0 \cos \gamma (\cos \theta \cos \gamma + \sin \gamma \cos \phi),$$

and since in the process of averaging the terms involving  $\phi$  will obviously vanish, they may be omitted from the first.

Hence, the probability of a collision being proportional to the relative velocity, the average energy exchanged between a P and a Q at impact

$$= \frac{2PQ}{P+Q} \frac{\int_0^\infty d\bar{v} \int_0^\infty dv_0 \int_0^\pi d\theta \int_0^\pi d\gamma \epsilon^{-a\bar{v}^2 - bv_0^2 - 2c\bar{v}v_0 \cos \theta} \bar{v}^2 v_0^3 \sin \theta \cdot v_0 \sin \gamma \cos \gamma \cdot \bar{v} v_0 \cos^2 \gamma \cos \theta}{\int_0^\infty d\bar{v} \int_0^\infty dv_0 \int_0^\pi d\theta \int_0^\pi d\gamma \epsilon^{-a\bar{v}^2 - bv_0^2 - 2c\bar{v}v_0 \cos \theta} \bar{v}^2 v_0^2 \sin \theta \cdot v_0 \sin \gamma \cos \gamma}.$$

Performing the integrations with respect to  $\theta$  and  $\gamma$ , this becomes

$$\frac{PQ}{P+Q} \frac{\int_0^\infty \int_0^\infty \epsilon^{-a\bar{v}^2 - bv_0^2} \left[ \bar{v}^2 v_0^3 \cosh 2c\bar{v}v_0 - \frac{\bar{v}v_0^2}{2c} \sinh 2c\bar{v}v_0 \right] d\bar{v} dv_0}{\int_0^\infty \int_0^\infty \epsilon^{-a\bar{v}^2 - bv_0^2} \bar{v} v_0^2 \sinh 2c\bar{v}v_0 \cdot d\bar{v} dv_0}.$$

[No element of the integral in the numerator can be negative: hence it can only vanish if

$$\cosh 2c\bar{v}v_0 - \frac{1}{2c\bar{v}v_0} \sinh 2c\bar{v}v_0 = 0$$

always, *i.e.* if  $c = 0$ .]

The integrals involved all depend on

$$\int_0^\infty \int_0^\infty e^{-a\bar{v}^2 - b\bar{v}v_0^2} \cosh 2c\bar{v}v_0 \cdot v_0 d\bar{v} dv_0,$$

which when  $ab - c^2$  is positive, as it is in this case, can at once be shown to be equal to

$$\frac{\sqrt{\pi a}}{4(ab - c^2)}.$$

Finally then, the average energy exchanged at an impact

$$\begin{aligned}
 &= \frac{PQ \left[ \frac{1}{4} \frac{d^2}{dc^2} - \frac{1}{4c} \frac{d}{dc} \right] \cdot \frac{1}{ab - c^2}}{P + Q \cdot \frac{1}{2} \frac{d}{dc} \cdot \frac{1}{ab - c^2}} \\
 &= \frac{2PQc}{(P + Q)(ab - c^2)}. \\
 &= \frac{2PQ}{(P + Q)^2} \left[ \frac{Q}{k} - \frac{P}{h} \right]
 \end{aligned}$$

5. On some Glass Globes with Internal Cavities produced during Cooling. By J. T. Bottomley.

(Abstract.)

The object of this communication is to exhibit and describe to the Royal Society a number of flint-glass globes having remarkable internal cavities produced during cooling. Along with these globes there is exhibited for comparison one globe having no perceptible internal cavity.

The making of these globes was shown to me by Mr John Griffin, manager of the St Rollox Flint Glass Works, Glasgow.

The globe having no internal cavity is marked A. A set of globes, four in number, are marked B<sub>1</sub>, B<sub>2</sub>, B<sub>3</sub>, B<sub>4</sub>, and the sixth globe is marked C.

The globes which have cavities were formed in the following way:—A pot of the finest glass having been selected, very free from any appearance of scum or bubbles, a ball of glass was gathered in the usual way at the end of the glass-blowers' iron or long blowing tube, and it was worked with the help of a wooden mould into the form of a glass ball projecting out, by a short neck of glass, from the end of the iron. When the red hot ball had been thoroughly examined to see that there was no flaw of any kind in the mass, the operator quickly brought it to the open window, and held it in the draught which was blowing sharply into the glass-house. The ball was at this time very bright red hot—glowing, in fact—and it was quietly turned round and round in this cold position, so as to cool equally all round.

At first it looked perfectly uniform throughout, but soon there

became visible in the middle of the mass a few very minute bright points—little specs it seemed—and these quickly grew, and were then perceived to be hollow spaces in the midst of the glass, and finally they assumed the appearance of the large bubbles now to be seen.

The conclusion of the operation was the cutting across of the neck of glass which supported the globe, and allowing the globe to drop off, and the passing of it through the *leer* or annealing furnace in the ordinary way. The place where the neck was broken off was finally ground flat by the polishers.

The explanation of the cavities is obvious. The sudden cooling of the outside of the glass globe caused the outer layer to become rigid, while the interior mass was still extremely hot and molten. But when the cooling reached the interior, that portion underwent great contraction of volume, and as the outer skin, which had enormous strength, on account of its shape, refused to be drawn in, the interior was forced to part somewhere, and these cavities were formed.

To follow a little further this interesting phenomenon, my friend Mr Griffin was good enough to make for me a globe without cavities, by allowing the whole mass to cool more uniformly, and letting the skin fall in towards the centre along with the interior mass. Thus he produced the globe marked A.

The process, which, of course, is the ordinary one for producing large glass paper-weights, and articles of that kind where flaws or bubbles are considered as blemishes, consisted in very frequently putting the cooling globe into the mouth of the glass-pot, and thus warming up the outer skin sufficiently to keep it from becoming suddenly rigid. Thus the cooling was gradually carried on with frequent partial reheating of the surface skin.

On examining the globes marked B<sub>1</sub>, &c., it will be seen that the distribution of the *smaller* cavities is very curious in appearance. It will easily be noticed that these little cavities are distributed over concentric spherical surfaces (to speak roughly). The cause of this was not difficult to trace.

The glass worker, in gathering a ball at the end of his iron, is in the habit of taking from the glass-pot a small quantity of glass to begin with. He then draws out the point of the iron into the air

for an instant, allowing the glass to cool very slightly ; and immediately thrusts it back for another charge which covers the first. This is repeated several times, till a sufficient quantity of glass has been gathered, when he puts the whole mass back into the mouth of the pot, and turning it round and round heats it up preparatory to blowing.

Now, on considering the matter, it seemed probable that these little cavities would form at the surfaces thus produced and exposed successively to the air. Any dust which might fall on the surface would give a *starting place* for a cavity ; or a place where the contracting glass would part under the negative pressure produced in the way I have described above.

Accordingly, I asked Mr Griffin if he could manage to gather a ball of glass of considerable size, without bringing it out from beneath the cover of the glass-pot, and he very kindly made the attempt and soon succeeded, and produced the ball marked C, which, when cooled in the manner already described, exhibited three or four beautiful large cavities, but none of the small cavities which are possessed by the others.

I do not propose to enter at all into a discussion of what I may be allowed to call the lessons to be learned from a study of these phenomena, though there are several points which are well worthy of consideration.

I wish only to refer to two matters which may be thought of in this connection. The first of these is one which was pointed out to me by Mr W. H. Barlow, F.R.S., the engineer of the Tay Bridge, and a Fellow of this Society. Mr Barlow is a member of the Ordnance Committee, and he was greatly interested in examining these globes, and in thinking of the possibilities of similar flaws being produced during the cooling of large castings, such as those used in the construction of big guns.

The second question to which I would direct attention is, perhaps, one which is already in the minds of all who have looked at these globes. It is the case of the cooling of a body like our earth. It seems certain that if the interior of the earth is of material which shrinks in cooling, cavities such as these would of necessity be formed by the shrinkage of the interior parts after an outside shell has become rigid.



6. Investigations on the Malpighian Tubes and the "Hepatic Cells" of the Araneina; and also on the Diverticula of the Asteridea. By Dr A. B. Griffiths, F.R.S. (Edin.), F.C.S. (Lond. & Paris), *Principal and Lecturer on Chemistry and Biology, School of Science, Lincoln*; and Alexander Johnstone, F.G.S. (Lond. & Edin.), *Assistant Professor of Geology and Mineralogy, University of Edinburgh, &c.*

The present memoir is a continuation of those already published by one of us, on the physiology of the Invertebrata.

1. *Malpighian Tubes of Tegenaria domestica.*

The intestines of *Tegenaria domestica* form a tube-like body, which dilates into a short rectum, and into this rectum the Malpighian tubes open (fig. 1, A and B). The secretion obtained from a large number of these tubes yields uric acid. The secretion is neutral to test papers.

The secretion was examined chemically by two separate methods :—(a) The clear liquid from the Malpighian tubes was treated with a hot dilute solution of sodium hydrate. On the addition of hydrochloric acid a slight flaky precipitate was obtained after standing six hours. These flakes were seen (under the microscope) to consist of rhombic plates (figs. 2a) and other crystalline forms. When these crystals are treated with nitric acid and then gently heated with ammonia, the beautiful four-sided prisms of reddish purple murexide  $[C_8H_4(NH_4)N_5O_6]$  are obtained (fig. 2b). The crystals (another portion) produced by sodium hydrate solution were dissolved in a drop or two of sodium carbonate solution, and then poured upon a piece of filter paper moistened with a solution of silver nitrate, when a dark brown stain of metallic silver was produced : thus showing, according to the test of Schiff, the presence of uric acid.

(b) Another method used was as follows (for testing the secretion of the Malpighian tubes of *Tegenaria*) :—

The secretion was boiled in distilled water, and evaporated carefully to dryness. The residue obtained was treated with absolute

ethyl alcohol and filtered. Boiling water was poured upon the residue, and an excess of pure acetic acid added to the aqueous filtrate.

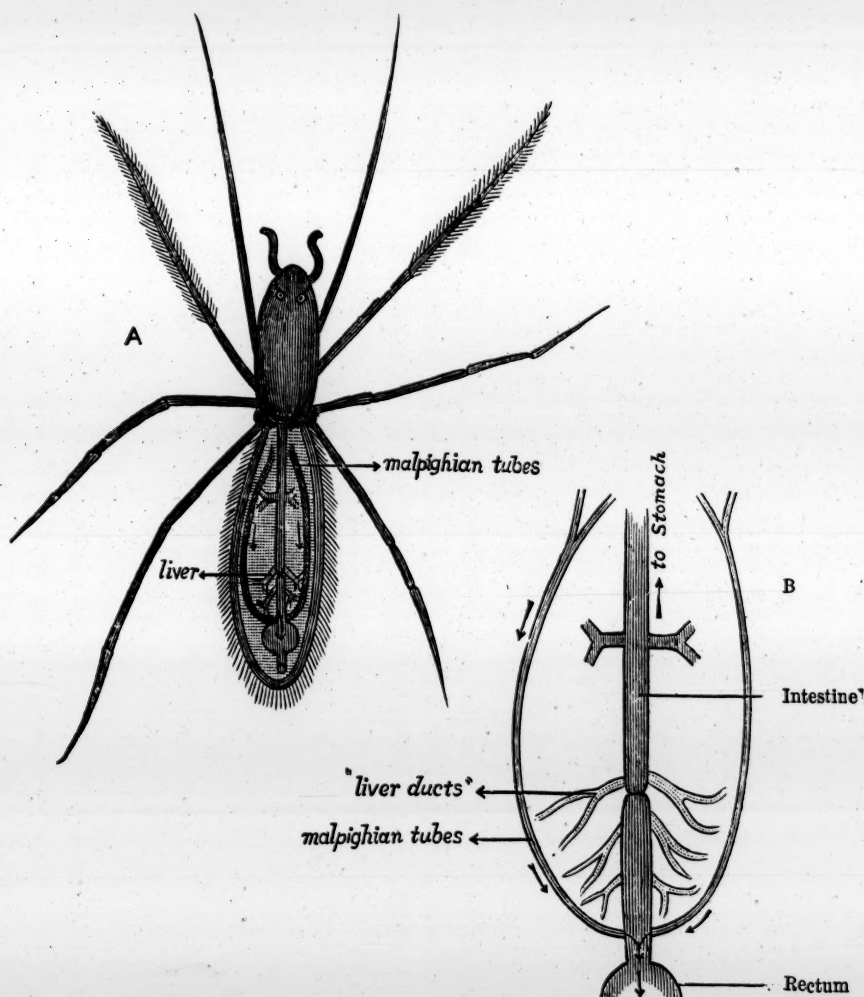


FIG. 1.—*Tegenaria domestica*. A dissection showing the position of the Malpighian tubes and the so-called "liver." A (greatly enlarged); B shows the same parts, but isolated from the abdomen.

After standing eight hours, crystals of uric acid made their appearance, and were easily recognised by the chemico-microscopical tests already mentioned.

The waste nitrogenous products of *Tegenaria domestica* are converted by the Malpighian tubes into uric acid; but the uric acid is *not* in that condition (*i.e.*, the acid condition), for it was found in combination with sodium. Sodium is easily found in the secretions

of the tubes ; therefore the secretion contains sodium urate. This fact also points to the inference that sodium is a constituent of the blood of *Tegenaria*.

No urea, guanin, or calcium phosphate could be detected in the secretion. This investigation proves the true renal function of the Malpighian tubes of the *Araneina*.

## 2. The Hepatic Cells of *Tegenaria domestica*.

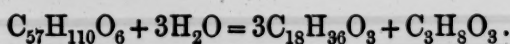
The "liver" ducts (hepatic cells) are to be found anteriorly to the rectum (fig. 1*a* and *b*), and pour their secretions into the ali-



FIG. 2.—*a* and *b*. Crystals of Uric Acid and Murexide. *a*, the uric acid crystals covered with a brown pigment ; *b*, Murexide crystals.

mentary canal (intestine). They appear under the microscope to consist of cellular tissue. The secretion of the "liver" of *Tegenaria domestica*, when perfectly fresh, gives an acid reaction to litmus paper. The following reactions were obtained from the secretions of a very large number of animals :—

1. The secretion forms an emulsion with oils yielding subsequently fatty acids and glycerol.
2. The secretion decomposes stearin, with the formation of stearic acid and glycerol—



3. The secretion acts upon starch paste with the formation of dextrose. The presence of dextrose was proved by the formation of brownish red cuprous oxide from Fehling's solution.

4. The secretion dissolves coagulated albumin (hard white of an egg).

5. Tannic acid gives a white precipitate from the secretion.

6. When a few drops of the secretion of the organ were examined chemico-microscopically, the following reactions were observed:— On running in between the slide and the cover-slip a solution of iodine in potassium iodide, a brown deposit is obtained; and on running in concentrated nitric acid upon another slide containing the secretion, yellow xanthoproteic acid was formed. These reactions show the presence of albumin in the secretion of the organ in question.

7. The presence of albumin in the secretion was further confirmed by the excellent tests of Dr R. Palm (*Zeitschrift für Analytische Chemie*, vol. xxvi. part 1).

8. The soluble zymase (ferment) secreted by cellular tubes was extracted according to the Wittich-Kistiakowsky method (Pflüger's *Archiv für Physiologie*, vol. ix. pp. 438–459). The isolated ferment converts fibrin (from the muscles of a young mouse) into leucin and tyrosin.

9. The albumins in the secretions are *not* converted into taurocholic acid or glycocholic acid; for *not* the slightest traces of these biliary acids could be detected by the Pettenkofer and other tests.

10. The secretion contains *approximately* 4 per cent. of solids, of which we could detect sodium. The slight residue (solids) effervesced on the addition of a dilute acid.

11. No glycogen was found in the organ or its secretion. From these investigations the so-called "liver" of the *Araneina* is similar in physiological functions to the pancreas of the *Vertebrata*.

### 3. *The Diverticula of the Asteridea.*

The saccular diverticula of *Uraster rubens* (fig. 3) have also been examined by similar reactions to those applied to the reactions of the "hepatic cells" of the *Araneina*, and with the same results.

Therefore, we conclude that the diverticula of the *Asteridea* are pancreatic in function.



It appears from the investigations already published by one of us on the so-called "livers" of the *Invertebrata*, that the pancreas was

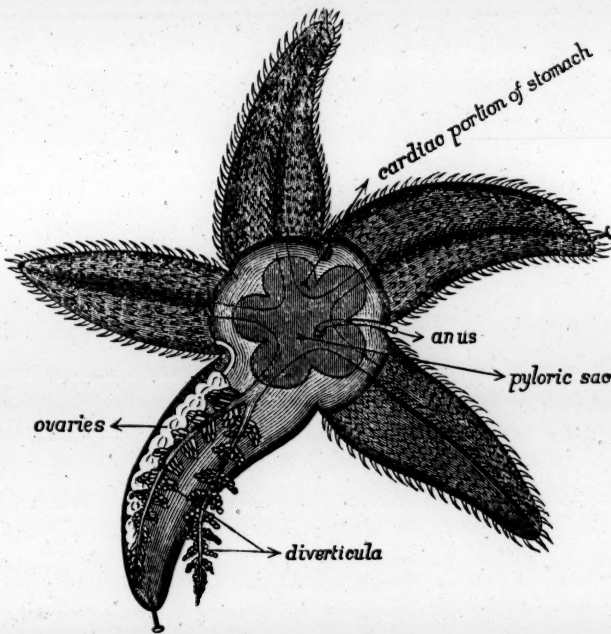


FIG. 3.—*Uraster rubens*. Dissection from dorsal or aboral aspect, showing the diverticula (2 lobes) and the pyloric sac.

the chief digestive organs in the early forms of animal life ; for, as far as these investigations have progressed, there seems to be no organ in the *Invertebrata* to answer to the liver of higher forms.

#### 7. On the Thomson Effect in Iron. By Prof. Tait.

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Monday, 16th January 1888.

PROFESSOR CHRYSTAL, LL.D., Vice-President,  
in the Chair.

The following Communications were read:—

1. Obituary Notices of former Vice-Presidents of the  
Society.

2. Problem in Relationship. By Professor  
A. Macfarlane, D.Sc.

The following problem was sent me by Mr Kirkman, F.R.S.; its solution illustrates the Calculus of Relationship, *Proc. Roy. Soc. Edinb.*, vol. xi. pp. 5 and 162:—

Two boys, Smith and Jones, of the same age, are each the nephew of the other; how many legal solutions?

The statements are that Smith is the nephew of Jones, and Jones the nephew of Smith. Let  $c$  denote *child*, then  $\frac{1}{c}$  denotes *parent*, and the equations are

$$S = cc\frac{1}{c}J \text{ and } J = cc\frac{1}{c}S.$$

Hence 
$$S = cc\frac{1}{c}cc\frac{1}{c}S.$$

Let  $m$  denote male and  $f$  female; then all the varieties of relationship are obtained by introducing  $m$  or  $f$  before the second, third, fifth, and sixth symbols; for the sex of the first and fourth symbols is given to be  $m$ .

Thus there are *sixteen varieties in all*.

Of these  $mmmm$ ,  $mffm$ ,  $fmmf$ , and  $ffff$  are CHRONOLOGICALLY impossible, because they make a man his own grandson, or a woman her own granddaughter. For example,

$$S = cmcf\frac{1}{c}cfcm\frac{1}{c}S,$$

reduces to

$$S = cmcfcm\frac{1}{c}S,$$

i.e., 
$$m\frac{1}{c}S = mcfc\left(m\frac{1}{c}S\right),$$

i.e., the father of S is his own grandson.

Again, there are eight which are LEGALLY impossible, because a person may not marry his or her grandparent, namely,

$$mmmf, mmfm, mfmf, fmmm, \\ fffm, ffmf, fmff, mfff.$$

For example,

$$S = cmcm\frac{1}{c}cmcf\frac{1}{c}S,$$

$$\therefore S = cmcmcf\frac{1}{c}S,$$

$$\therefore m\frac{1}{c}S = mcmcfc\frac{1}{c}S,$$

i.e., the father of S marries his own grandmother.

There remain the four cases

$$mmff, mfmf, ffmm, fmfm;$$

each of which is legally and physiologically possible. For there is nothing to prevent two persons, each twenty years old say, from marrying each the appropriate parent of the other, each of whom may be forty years old, and the Smith and Jones of the problem may be the result of these contemporaneous marriages. For example, take the first of these,

$$S = cmcm\frac{1}{c}cfcf\frac{1}{c}S$$

$$\therefore m\frac{1}{c}\left(m\frac{1}{c}S\right) = m\frac{1}{c}cfcf\left(f\frac{1}{c}S\right);$$

i.e., the father of Smith's father marries the daughter of Smith's mother.

Thus there are four solutions—(1) Two men marry each the mother of the other. (2) Two women marry each the father of the other. (3) A man and a woman marry the mother and father of one another, which comprehends the two cases of Smith being the son of the old woman, and Smith being the son of the young woman.

*Note.*—Here Smith and Jones are taken as arbitrary names, equivalent for example to Tom and Hugh. If the convention about surnames is taken into account—that a child's surname is identical with that of his father—then only the first and second solutions are possible.

### 3. On Transition-Resistance and Polarisation.

By W. Peddie, B.Sc.

In a paper communicated to this Society last Session, I gave an empirical formula representing the relation between current-strength and time when platinum plates are used for the passage of a current through acidulated water, the electromotive force being that of a single Daniell cell. As is well known, we can look upon the electrodes as condensers of very great capacity. If  $E$  be the electromotive force of the battery used, while  $e$  is the reverse electromotive force of polarisation, and  $r$ ,  $x$ , and  $c$  are the values of the resistance, the current-strength, and the capacity respectively, the equations of conduction through the condenser are as follows:—

$$E - e = rx$$

$$x = \frac{e}{R} + c \frac{de}{dt},$$

$R$  being the resistance of the dielectric. If, in the case we are considering, no decomposition of the liquid occurs, we may suppose  $R$  to be infinite. Hence the second equation becomes

$$x = c \frac{de}{dt},$$

and we get for the law connecting current-strength and time, the relation

$$x = x_0 e^{-\frac{t}{cr}},$$

where  $x_0$  is the value of  $x$  when  $t = 0$ . But the curve represented by this equation does not represent the actual variation of  $x$  with time. Hence, either  $r$  or  $c$  must vary, or both must vary simultaneously.

In a paper also communicated last Session, I showed that there is a very considerable transition-resistance at the surface of platinum, and that this resistance goes on slowly increasing as the time that has elapsed after heating the platinum to redness increases, but the law of increase is unknown. I have made the assumption that the rate of increase is proportional to the excess of the final value of the resistance over its value at the given time. If we use  $R$  to



represent the total increase of transition-resistance in time,  $t$  this gives

$$R = R_0(1 - \varepsilon^{-bt}), \quad . \quad . \quad . \quad . \quad . \quad . \quad (A)$$

where  $R_0$  is the final value of  $R$ . Hence, if we delete the suffix, and write  $r_0$  for the value of the total resistance in the circuit when  $t = 0$ , we get for the value of the total resistance at any time

$$r = r_0 + R(1 - \varepsilon^{-bt}).$$

So, if we assume the capacity to be constant, the equations of conduction give approximately with this value of  $r$ ,

$$x(1 + a(1 - \varepsilon^{-bt})) = x_0 \varepsilon^{-kt}, \quad . \quad . \quad . \quad . \quad (B)$$

where

$$a = \frac{R}{r_0}, \quad k = \frac{1}{c(R + r_0)},$$

$c$  being the capacity of the condenser. The approximation is obtained by assuming that the reciprocal of the quantity  $cb(r_0 + R)$ , is very small compared with unity. Since  $C$  and  $R$  are very large quantities, while  $b$  is not very small, this assumption may safely be made.

This equation contains four unknown constants, and their numerical value cannot be determined by elimination, for the degree of the resulting equation is too large. So I have obtained them by giving probable values to  $a$  and  $x_0$ , and then calculating  $b$  and  $k$ . In this way, by giving different values to  $a$  and  $x_0$ , curves were obtained between which the observed curve lay, and so by varying  $a$  and  $x_0$  satisfactory values were finally obtained. The curve to which I have applied the equation is that drawn through the group of points marked  $a$  in the plate which accompanies my paper already referred to on transition-resistance. The value of the constants were  $a = 10$ ,  $x_0 = 420$ ,  $b = 0.3924$ ,  $k = 0.0404$ . The calculated and observed values of  $x$  for different values of  $t$  are given below.

$t$	1	2	4	8	12	16
$x(\text{observed}),$	93	60	40.4	28.4	23	20.2
$x(\text{calculated}),$	95	60.2	40.8	28.8	23.7	20

The coincidence is obviously extremely close. I also applied the equation to the curve marked  $b$  in the plate alluded to, assuming the same values for  $b$  and  $k$ , which is at least approximately correct. The values of  $a$  and  $x_0$  are 5.3 and 230. The results are

$t$	1	2	4	8	12
$x(\text{observed}),$	83	53.5	38.6	27.3	22.3
$x(\text{calculated}),$	81.9	55.1	37.9	26.5	22.9

the coincidence again being close, though not so good as before.

The value of the constant  $b$  shows that in at most twenty minutes (the unit in terms of which the constants are calculated being one minute), the quantity  $\varepsilon^{-bt}$  practically reaches its final value; that is, the resistance has practically reached its final value. This is not in accordance with the idea on which the above investigation was based. But the close agreement of calculation and experiment renders it probable that the resistance may increase more rapidly when polarisation is going on, than when the plates are merely having oxygen slowly deposited on them. I have remarked in my paper on transition-resistance, that certain experiments I made seemed to indicate this. To obtain definite information on the point, I have used a dead beat galvanometer, so that the reading of the deflection could be taken at an interval of five seconds after starting the current. In all other points the arrangement of the apparatus was the same as in the previous experiments; that is, the platinum electrodes 60 square centimetres in area, which dipped into a dilute solution of sulphuric acid, were connected with the terminals of the galvanometer. A single tray-Daniell cell was placed in circuit when required. When the cell was joined in, readings were taken at intervals of five seconds for a short time. Then the plates were polarised until the reverse electromotive force was as nearly as possible equal to that of a Daniell cell, after which the battery was thrown out of circuit, and the readings during discharge of the plates were taken as before.

*Experiment 1a.*

<i>Time in seconds,</i>	5	10	15	20	25	30
<i>Deflection during charge,</i>	8	5.7	4.6	4	3.5	3.2
<i>Deflection during discharge, ...</i>	3.64	2.94	...	2.64	2.34	

*Experiment 2a.*

<i>Time in seconds,</i>	5	10	15	20
<i>Deflection during charge,</i>	9.03	7.03	6.03	5.43
<i>Deflection during discharge,</i>	4.7	3.8	3.3	3.2

*Experiment 3a.*

<i>Time in seconds,</i>	5	10	15	20
<i>Deflection during charge,</i>	9.38	6.38	5.38	4.68

These three experiments were performed in the order indicated on the same day. The next experiment was made on the following day.

*Experiment 4a.*

<i>Time in seconds,</i>	10	15	20	25
<i>Deflection during charge,</i>	8.6	6.5	5.5	4.8
<i>Deflection during discharge,</i>	3.41	2.81	2.51	2.21

In another experiment care was taken to ensure that the electromotive force of the battery had the same value when the plates were to be charged as it had when thrown out of the circuit previous to discharge of the plates. The following table gives the result:—

*Experiment 5a.*

<i>Time in seconds,</i>	5	10	15	20	25
<i>Deflection during discharge,</i>	3.98	2.98	2.38	2.08	1.93
<i>Deflection during charge,</i>	7.35	5.85	4.95	4.45	4.05

These experiments show that the magnitude of the transition-resistance is about doubled when the plates are polarised fully by a Daniell cell. They were all made when the plates had been unheated for some weeks. Corresponding experiments were made after the plates had been heated, and little difference could be detected in the values of the initial deflections during charge and discharge. This shows that the difference formerly observed was due to the transition-resistance. The only cause of uncertainty lies in the fact that, in the time which elapsed between taking out the battery and connecting the plates, the electromotive force of polarisation might have largely diminished. This interval was only a small fraction of a second. But the polarisation does not so diminish, for the initial value of the depolarising current was found to be the same even if the discharge was commenced one minute after breaking the circuit. When the plates were heated a resistance of about 600 ohms had to be placed in the circuit to reduce the deflections to their value before heating.

In the formula (B), the constant  $\alpha$ , which represents the ratio of

the final to the initial resistance, has the value 10. But we have just seen from experiment that the true value of that ratio is 2. Hence the close correspondence between the curve represented by the equation (B), and the observed curve merely shows that the other causes of variation in the current strength are of such a nature that their effects can be represented as due to variation in the value of the transition-resistance, according to the law (A): so that in equation (A), the constant  $R_0$  does not give the true final value of transition-resistance.

One cause of variation of the strength of the current of which I have taken no account, consists in the decomposition of the liquid by smaller electromotive forces than are necessary for visible decomposition, the products of decomposition being dissolved in the liquid.

Also no account has been taken of any variation of capacity which may occur. Now Varley has shown that the capacity of such an arrangement as we are dealing with, increases as the electromotive force increases. He used one of the ordinary methods of determining the capacity of a condenser. It is easy to show the same effect by the methods I have been using. If the circuit including an electrolyte and a battery be broken, for a short time, during the process of charging the plates, and be again completed, the current is found to be stronger. This shows that the capacity of the plates has increased in the interval, thus diminishing the reverse electromotive force of polarisation. In the following experiment readings were taken at intervals of five seconds, and a break was occasionally made in the circuit. The interval of break was half a minute.

*Experiment 1b.*

*Deflection.* — 7·81, 6·41, 5·31, 4·71, 4·21, 3·81, — 4·11, 3·41, 3·11, 2·91, 2·71, 2·59, — 3·11, 2·71, 2·47, 2·33, 2·24, 2·15, — 2·51, 2·26, 2·12, 2·03, 1·96, 1·91, — 2·16, 1·91, 1·84, 1·79, 1·75, — 1·86, 1·76, 1·71, 1·67, — 1·73, 1·66, 1·58, 1·55, 1·52, 1·47, 1·45, 1·42, — 1·61, 1·51, 1·46, 1·44, 1·40, — 1·54, 1·46, 1·41, 1·37, 1·36.

The same phenomena appear during discharge of the plates, the increase of current-strength after the break being due, in this case, to the decrease of capacity causing increase of the electro-



motive force of polarisation. The results are given below. The last interval of break was 1 minute. All the other intervals were half a minute.

*Experiment 2b.*

*Deflection.* — 4.30, 3.60, 3.10, 2.84, 2.60, 2.45, 2.34, 2.20, 2.12, 2.04, 2.00, 1.92, 1.87, 1.82, 1.78, 1.74, 1.70, — 1.70, 1.60, 1.56, 1.52, 1.49, 1.47, 1.44, 1.42, 1.40, 1.38, 1.36, — 1.37, 1.34, 1.32, 1.30, 1.28, 1.26, 1.25, 1.24, 1.23, 1.22, 1.21, — 1.23, 1.20, 1.19, 1.17, 1.16, 1.15, 1.14, 1.13, 1.12, 1.11, 1.10, — 1.13, 1.11, 1.10, 1.09, 1.08, 1.07, 1.06, 1.06, 1.06, 1.05, 1.04, — 1.07, 1.05, 1.04, 1.03, 1.02, 1.02, 1.01, 1.01, 1.01, 1.00, — 1.02, 1.01, 1.00, 0.99, 0.99, 0.98, 0.98, 0.97, 0.96, — 1.00, 0.99, 0.98, 0.97.

In order to explain the increase of capacity, Varley (*Proc. Roy. Soc.*, 1871), supposed that there was a film of gas separating the plate from the liquid, and that this film was compressed by the tendency of the oppositely electrified surfaces to approach. Since the electric attraction is inversely as the square of the distance, he supposed that the molecular forces keeping the particles of the gas apart were inversely as the cube of the distance. I do not think that his supposition of compression is necessary; but, as he does not enter into detail regarding his method of experimenting, a conclusion cannot be easily arrived at. However that may be, the phenomena exhibited in experiments 2*a* and 2*b*, merely show that electric absorption is occurring in the one case, and that residual discharge occurs in the other. The insulating portion of the circuit consists of two parts,—the gaseous film which conducts appreciably, and the space between the gas and the plate, across which no conduction can occur so long as decomposition does not take place. Thus Clerk-Maxwell's theory of a composite dielectric applies to it, and shows that electric absorption and residual discharge must occur. The oppositely charged layers of electricity are not separated merely by the molecular distance between the gas and the plate until conduction has taken place through the gaseous film. This fact has important bearings on all determinations of the capacity of electrodes.

I give below the results of an experiment on the discharge of

the plates, no break being made in the circuit. The first reading was taken ten seconds after commencing the discharge.

*Deflection at intervals of five seconds.*—2·03, 1·43, 1·23, 1·10, 1·02, 0·93, 0·86, 0·81, 0·76, 0·71, 0·68, 0·64, 0·62, 0·59, 0·56, 0·54, 0·52, 0·51, 0·49, 0·48, 0·46, 0·45, 0·44.

*At intervals of ten seconds.*—0·42, 0·40, 0·38, 0·37, 0·35, 0·34.

*At intervals of one quarter of a minute.*—0·32, 0·30, 0·29, 0·28, 0·27, 0·25, 0·24, 0·24.

*At intervals of half a minute.*—0·22, 0·21, 0·20, 0·19.

*At intervals of one minute.*—0·16, 0·15, 0·13, 0·12, 0·12, 0·11.

If we take the values of  $x$  for  $t=10$  and  $t=20$ , and calculate from them the value of  $b$  in the equation

$$x = x_0 e^{-bt},$$

we get  $b=0\cdot05$ . This is the equation of the logarithmic curve which coincides with the observed curve when  $t$  is small. Hence  $b$  is approximately equal to the reciprocal of the product of the capacity and the resistance. Now, as already observed, the initial value of the transition-resistance when the *charging* current was started was about 600 ohms, and the final value was about double of this. Hence

$$b = \frac{1}{1200 C} = 0\cdot05.$$

To obtain the value of the capacity in electrostatic units, we must convert the resistance in ohms into its equivalent in electrostatic units. Thus gives

$$C = \frac{1}{6} \cdot 10^{11}.$$

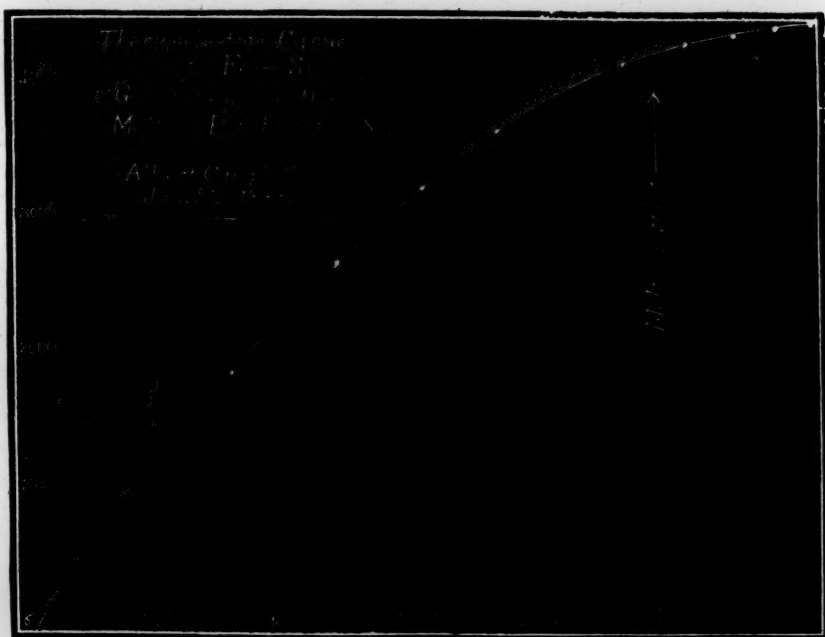
The effective area of the two plates may be taken roughly as 200 square centimetres. So the capacity per unit area is  $\frac{1}{12} \cdot 10^9$ . Hence the thickness of the dielectric is approximately  $10^{-9}$  cm. Sir W. Thomson's higher and lower limits for molecular distances are  $10^{-8}$  and  $\frac{1}{2} \cdot 10^{-9}$  respectively.

In conclusion, I have to acknowledge my indebtedness to Mr J. Butters for much valuable assistance, both in the experimental and numerical work.

*Added June 23.*—The simpler empirical formula  $t^n(x-a)=b$  may be used for purposes of calculation instead of (B) when  $t$  is not very large.

#### 4. The Change in the Thermoelectric Properties of Tin at its Melting Point. By Albert Campbell, B.A.

As in all the thermoelectric diagrams hitherto published the lines of the metals stop short at the melting points, it seemed interesting to the writer to trace the change in the thermoelectric properties of a metal as the temperature is raised to, and beyond, its melting point. The fusible metals containing bismuth or antimony were rejected, because their large expansion at the point of solidification rendered them very unmanageable; ordinary block tin was accordingly chosen.



The tin was contained in a thin glass tube (about half a metre long), one end of which was bent up at right angles to the remainder. This bent end, which was almost filled by the tin, was packed (vertically) in asbestos within two small copper cylinders, the longer part of the tube projecting from a hole in the side. At the point of emergence it was well protected from the hot copper by a thick wrapping of asbestos. Into the tin in the vertical part of the tube dipped the end (already tinned) of a thin iron strip. The other ends of this strip and of the tin in the glass tube were soldered to

copper wires proceeding to the commutator of a mirror galvanometer. These junctions (well varnished) were immersed in a large can of cold water, which was frequently stirred. A thermometer was placed with its bulb almost touching the hot junction. The heating was done by a small spirit-lamp underneath the copper cylinders, and the temperature was almost perfectly steady at each measurement. The resistance of the galvanometer circuit was made very large compared with that of the tin and iron which were heated. Thus the change of resistance of the glass tube and of the tin in melting might be neglected. As will be seen, the cold temperature rose slowly; this small rise of temperature was not taken into account. The deflections were each the mean of four (two to each side of the scale).

It was found that up to at least  $226^{\circ}\text{C}$ . the thermometer curve was *very nearly* a parabola. The last column in the table below gives the values of the deflection (D), calculated from the formula,

$$D = 14\cdot16\theta - \cdot0207\theta^2$$

where  $\theta$  = temperature of hot junction  $-5^{\circ}\cdot6^{\circ}\text{C}$ . As will be seen by the table, above  $226^{\circ}\text{C}$ . there is a marked divergence from the calculated values. Thus it seems that above the melting-point the tin line, instead of meeting the iron line at about  $342^{\circ}\text{C}$ . becomes almost parallel to the latter. Further investigation, however, at higher temperatures, seems desirable.

TABLE.

Temperature C.		Deflection.	
Cold.	Hot.	Observed.	Calculated.
$5^{\circ}\cdot6$	$81^{\circ}\cdot2$	952	952
$5^{\circ}\cdot7$	$119^{\circ}\cdot1$	1346	1346
$5^{\circ}\cdot8$	$148^{\circ}\cdot7$	1607	1602
$6^{\circ}\cdot0$	$179^{\circ}\cdot7$	1837	1837
$6^{\circ}\cdot1$	$226^{\circ}\cdot5$	2100	2114
$6^{\circ}\cdot2$	$251^{\circ}\cdot4$	2162	2229
$6^{\circ}\cdot3$	$268^{\circ}\cdot6$	2196	2292
$6^{\circ}\cdot4$	$285^{\circ}\cdot8$	2221	2342
$6^{\circ}\cdot5$	$297^{\circ}\cdot8$	2227	2369



## 5. On the Thermoelectric Properties of Iron.

By Prof. Tait.

For some time Signor Battelli has been engaged, with remarkable success, in measuring directly the amount of the "Thomson effect" in various metals.

With the exception of iron, the common metals have given him results coinciding as closely as could be expected with those I found in 1872 by an indirect method. Among other particularly satisfactory things, he has directly verified the first of the two changes of sign of the Thomson effect in nickel. And I think it will be allowed that what I introduced long ago as a mere working hypothesis,—that *the Thomson effect is directly proportional to the absolute temperature*,—if it was not completely established as a fact by my own experiments, has been made absolutely certain by the recent work of Campbell and of Battelli.

In his paper on iron, however, he finds that the specific heat of electricity in that metal is by no means closely proportional to the absolute temperature. I had long ago met with the same difficulty, and in fact I have never found two specimens of iron—even when cut from the same hank of wire—which agreed well with one another. Even Matthiessen's pure iron does not give a straight line on the thermoelectric diagram (for temperatures under low red heat); and, as will be seen in Pl. IX., *Trans. R.S.E.*, vol. xxvii., the lines for various kinds of iron are all sinuous, but all unlike one another. In 1870 I stated that, iron being one member of the thermoelectric couple, "the parabola was slightly steeper on the hotter than on the colder side." This implies that, if the line of the other metal be straight, the line of iron is concave *downwards*, as the "diagram" is drawn. Sig. Battelli gives the following values of the Thomson effect, which agree with this statement, viz.:—At 53° C.  $-9.2.10^{-6}$ ; at 108° C.  $-12.15.10^{-6}$ ; at 242° C.  $-17.10^{-6}$ ; and at 308° C.  $-21.4.10^{-6}$ . The fact seems to be that not only is iron never obtained pure, but it is one of those metals on whose physical properties a very slight impurity produces a marked effect.

Sig. Battelli kindly sent me one of the two iron rods employed in his experiments; and Messrs Shand and Morison have determined

its thermoelectric properties for me by the indirect method. The following is their account of the work :—

“The experiment was carried out by heating two junctions, of PdCo and FeCu, joined together at the hot end, to a red heat by means of a cylinder of red hot iron ; the other ends being immersed in separate quantities of cold water (same temperature as the atmosphere). The rise in temperature of the cold junctions as the experiment went on was extremely small. A correction for this made no difference on points on the curve, and was therefore neglected. The currents from the two junctions were made to pass through the galvanometer in opposite directions ; and the deflections were alternately observed, from the PdCo and FeCu circuits, at equal intervals of time. At the higher temperatures readings were noted down as soon as the spot of light on the scale came to rest ; and observations were corrected by taking the mean of two consecutive readings of the PdCo as corresponding to the deflection of the FeCu between them.

“The next experiment was to find the thermometric value of the PdCo deflections. The same junctions were now heated, by means of an oil-bath, and the temperature observed by a thermometer, up to 230° C., in the case of Battelli's iron up to 250° C. The deflections were observed every 10° from 30° upwards ; observations being also taken as the temperature fell (the duplicate readings coincided very closely). In order that the junction and thermometer should have the same temperature, when the readings were taken, heat was applied by means of a Bunsen burner until the mercury rose 5, and in some cases 6, 7, 8 and 9 degrees (according as the temperature became higher), above the temperature of last reading ; it was then removed, the oil during the whole time being vigorously stirred, the mercury continued to rise slowly, and finally reached the proper degree for the observation. The curve for PdCo was drawn, and constants were calculated, by means of which points on the curve at higher temperatures were found, the curve in this way being extended to about 600° C. By the equation to the PdCo parabola points were verified, and found to coincide, very closely, in some cases exactly, with those observed.

“The PdCo curve was tested by lessening the resistance in the PdCo circuit, and the new curve thus found was compared in detail with the previous one.

"The FeCu deflections were now plotted against the PdCo deflections, and the constants of the curve calculated, points being verified as before.

"The experiments on 'Battelli's iron' were carried out in the same way. The two FeCu curves were then compared, with the following results.

"Ratio of distance between origin and neutral point, to distance between neutral and vanishing points :—

	Ordinary Iron.	Battelli's Iron.
Observed	1 : 0·75	1 : 0·79

"If we assume the specific heat in iron to be expressed by  $k_1 t + lt^2$ , while that in copper is  $k_2 t$ , we find from the curves that  $k_1$  and  $l$  are both negative ; and we have

$$\text{Ratio of } l \text{ to } (k_1 - k_2) \begin{cases} \text{Ordinary iron} & 0\cdot0011 \\ \text{Battelli's} & \text{,, } 0\cdot0021. \end{cases}$$

## 6. On the Constitution of Dielectrics.

By W. Peddie, B.Sc.

(*Abstract.*)

In this paper the author pointed out that the experiments detailed in his preceding paper on Transition-resistance and Polarisation, show that, whatever be the ultimate nature of Dielectrics, any insulator upon which a film of gas is condensed must exhibit the phenomena of electric absorption and residual discharge.

## 7. On Mr Omond's Observations of Fog-Bows.

By Prof. Tait.

The author remarked that one of the constituents of the *double* fog-bow described in some of Mr Omond's recent observations, is obviously the ordinary primary rainbow, diminished in consequence of the very small size of the water drops. But the other, having nearly the same radius but *with its colours in the opposite order*, appears to be due to ice-crystals in the fog. This is quite con-

sistent with the record of temperatures. Just as small drops of water may remain unfrozen in air below  $0^{\circ}$  C., small ice crystals may remain unmelted at temperatures above that point.

### 8. Letter from the Astronomer-Royal.

By permission of the Meeting, a letter was read from the Astronomer-Royal for Scotland on the fall of a part of the cliff below Nelson's Monument.

#### PRIVATE BUSINESS.

Mr John Norman Collie, Mr R. E. Allardice, Mr F. Grant Ogilvie, Mr D. F. Lowe, and Mr Charles Hunter Stewart were balloted for, and declared duly elected Fellows of the Society.

*Monday, January 30, 1888.*

The REV. PROFESSOR FLINT, D.D., Vice-President,  
in the Chair.

The following Communications were read:—

1. On the Causes of Movements in General Prices.  
By Professor Nicholson.

2. A New Method for Preserving the Blood in a Fluid State outside the Body. By Prof. John Berry Haycraft, and E. W. Carlier, M.B.

A grant was made by the British Medical Association, on the recommendation of the Scientific Grants Committee of the Association, towards the expenses of a research, a part of which appears in this communication.

Dr Freund and Professor Haycraft, working independently, have succeeded in keeping blood in a fluid state when removed from the body. In principle both these methods were the same, the blood



being received into fluids having surface-tensions which differ from it, such as oil and paraffin.

These experiments bring one to the threshold of an inquiry as to what can be the action of a chemically inert solid—such as glass or porcelain—when it produces by mere contact such important changes in the blood. Methods already discovered for keeping blood fluid when removed from the body cannot be applied to the human subject, and it was our wish to obtain some method by means of which we could experiment with our own blood, and one which might be available clinically. The following method exceeded the anticipations we had formed of it, for we have only on one occasion failed to keep blood in a fluid state for half-an-hour to an hour—as long, in fact, as our experiments lasted.

A cylindrical vessel, about 1·6 inch wide and about a foot long, is filled with castor oil. The finger of the experimenter is greased and plunged within the oil. It is then pricked, and the exuding droplets gradually sink in the vessel. This is then filled to the brim, the finger having been withdrawn, and it is covered by a slip of glass. When the droplets have nearly reached the bottom of the vessel—in about fifteen minutes—it is inverted, and the drops will again fall. It may be inverted again and again as desired. In this way the drops are prevented from coming in contact with the walls of the vessel. After about five minutes the droplets separate each one into two layers. The corpuscles sink to the bottom, and form a red layer, and the plasma remains at the top. If the drops be withdrawn from the oil after half-an-hour or so, they are seen to be perfectly fluid, coagulating, however, some three or four minutes after they have come in contact with solid matter.

### 3. The Formula of Morphine. By R. B. Dott, F.I.C., and Ralph Stockman, M.D.

(From the Materia Medica Department, Edinburgh University.)

As the alkaloid morphine, discovered in 1804, was the first substance of the kind known, it naturally received a good deal of attention. Some years later the new base was analysed by several distinguished chemists, but their results did not lead them to a formula accurately indicating the composition of the alkaloid. To

Laurent\* belongs the honour of having given the formula  $C_{17}H_{19}NO_3$  (using the modern notation), the accuracy of which formula has been confirmed by subsequent analyses, particularly by those of Matthiessen and Wright.† Nothing was ever observed to suggest that the formula is a multiple of these members until Wright, after an elaborate investigation of the derivatives of morphine and codeine, came to the conclusion that the true formula must be at least double the empirical, and that we ought therefore to write  $C_{34}H_{38}N_2O_6$ . The reasons which led Dr Wright to that conclusion are, briefly stated, as follows:—

It was found that when morphine is heated with hydrochloric acid in a sealed tube,‡ there is produced, besides apomorphine, a mixture of amorphous bases. This mixture, when fractionated, appears by analysis to contain a base "R" ( $C_{34}H_{36}Cl_2N_2O_4$ ) homologous with chlorocodide, and also a base "P" ( $C_{34}H_{39}ClN_2O_6$ ). Now it is evident that if it were clearly established that the latter base contains only 1 atom of Cl to the 34 of C, and if at the same time it were proved that no polymerisation had taken place in its formation, the formula of morphine must be  $C_{34}H_{38}N_2O_6$ , and not  $C_{17}H_{19}NO_3$ . Similarly, chlorocodide is apparently preceded in its formation by a base homologous with base "P," and as chlorocodide yields codeine by the simple action of water, it is manifest that no polymerisation has taken place. The formula of codeine must therefore be  $C_{36}H_{42}N_2O_6$ ; and as codeine is simply the methyl ether of morphine, it follows that morphine must be  $C_{34}H_{38}N_2O_6$ . The other argument for the higher formula is deduced from a study of the acetyl derivatives of morphine. In the course of an extended investigation of these bodies, § Wright obtained what he describes as monoacetylmorphine [ $C_{34}H_{37}(C_2H_3O)N_2O_6$ ]. If we were certain that a derivative of that composition had been formed, and that its formation was unaccompanied by polymerisation, the proof for the higher formula would be complete.

In preparing certain of the morphine derivatives for the purposes of pharmacological investigation, we have obtained results which lead us to a different conclusion regarding the formula from that

\* *Am. Ch. Phy.*, lxii. 96.

† *Proc. Roy. Soc.*, xvii. 455.

‡ *Proc. Roy. Soc.*, xvii. 460; xviii. 83.

§ *Jour. Chem. Soc.*, [2] xii. 1031.

arrived at by Wright, and our experiments are therefore described in the present paper.

(1) *Chlorocodide*.—This substance was prepared exactly in the manner described by Matthiessen and Wright. Although white or nearly so when freshly precipitated, it became of a pale green colour on exposure to the air. The chloroplatinate dried at  $120^{\circ}$ , yielded on ignition 18.43 per cent. Pt. Wright obtained 18.60 per cent. Pt. The percentage required by the formula  $C_{36}H_{40}Cl_2N_2O_4.PtH_2Cl_6$ , is 18.81. On uniting the first and third fractions obtained in purifying the base, the mixture gave a chloroplatinate which yielded 17.97 per cent. Pt. As regards physiological action, the second fraction nearly resembled apomorphine, while the mixed fractions rather approached codeine in its action on animals. The fact that the so-called chlorocodide and its hydrochloride are non-crystalline, shows that it is something other than a mixture of apomorphine and codeine, and that conclusion is confirmed by the analytical results. These results, however, indicate that chlorocodide is not obtained in the pure state by the authorised process, and that the substance is probably a mixture of bases, most likely of a chlorinated and non-chlorinated base. Indeed, it is very difficult to derive any definite information from an amorphous mixture, such as that produced by the action of hydrochloric acid on morphine or codeine. There is really no evidence to prove that a compound containing 1 atom of chlorine to 34 atoms of carbon has been formed from morphine, or that a similar product has been obtained from codeine.

(2) *Acetylmorphine*.—Wright describes four acetyl derivatives as obtained by him from morphine. At present we are only concerned with the monoacetyl compound, represented by the formula  $C_{34}H_{37}(C_2H_3O)N_2O_6$ . It was prepared by the action of acetic anhydride on morphine, "when a considerably smaller quantity" of the anhydride is taken than is required to form diacetylmorphine. The product "resembles  $\beta$ -diacetylmorphine in every particular, save that it yields different numbers on analysis." The numbers given by Wright agree only fairly well with theory. "That the substance is truly a monoacetyl morphine, and not a mixture of morphine and diacetyl derivatives, is shown by the fact that the base itself is soluble in ether; whereas morphine is practically not soluble in that menstruum. Moreover, a mixture of  $\beta$ -diacetyl-

morphine hydrochloride and morphine hydrochloride in equal quantities dissolved in a little water, allows almost the whole of the latter salt to crystallise out, and does not dry up to a varnish over sulphuric acid, but to a crystalline mass wetted by a syrup, which finally dries up to a glaze over the crystals." Wright obtained from the chloroplatinate 19.45 per cent. Pt; the monoacetylmorphine compound requiring 19.29 per cent. of metal.

We have repeated Wright's experiments, with the results under-noted. 15 grams of morphine were dried at 120° C., and thoroughly mixed with 1.25 gram acetic anhydride. The mixture was then warmed on the water-bath for half an hour, treated with water and with sodium carbonate in slight excess, and the whole shaken up with ether. On separating and evaporating the ether a non-crystalline residue remained, to which dilute hydrochloric acid was added in quantity just sufficient to render faintly acid. The strong solution showed no signs of crystallisation even after the lapse of two days. To a portion of the solution platinic chloride was added, and the washed precipitate dried at 120° C. On ignition 0.124 gram gave 0.023 gram Pt = a yield of 18.54 per cent.

$C_{34}H_{36}(C_2H_3O)_2N_2O_6.PtH_2Cl_6$  gives 18.48 per cent. Pt.

$C_{34}H_{37}(C_2H_3O)N_2O_6.PtH_2Cl_6$  gives 19.53 per cent. Pt.

In this experiment there is no evidence that any monoacetylmorphine was formed, although the conditions were most favourable for its formation. The solution must have contained either diacetylmorphine or a mixture of tetracetylmorphine and morphine. Wright's assumption, that the supposed monoacetylmorphine could not have contained morphine, because the latter is insoluble in ether, is not well founded. Under certain conditions, probably when it is freshly precipitated and partially amorphous, morphine is soluble in ether. The evidence from the non-crystalline condition of the product is far from conclusive, as crystallisation is influenced by a variety of circumstances not well understood; and it must be remembered that tetracetylmorphine hydrochloride is a very soluble and not readily crystallisable salt, which may form a basic compound with morphine.

(3) *Ethylmorphine*.—Since Wright contributed his papers on morphine derivatives, it has been shown by Grimaux,\* that part of

\* *Comptes Rend.*, xcii.



the hydrogen in the morphine molecule may be replaced by alcohol radicals. We endeavoured to prepare a monoethyl derivative  $[C_{34}H_{37}(C_2H_5)N_2O_6]$ , by heating together equivalent quantities of morphine, soda, and ethyl iodide, in alcoholic solution. The alcohol having been evaporated, the residue was exhausted with chloroform, and the chloroform extract converted into hydrochloride. The resulting crystalline mass was pressed in calico, and a chloroplatinate prepared from the crystals. Dried at  $130^\circ$ , 0.44 gram gave on ignition 0.083 gram = 18.86 per cent. Pt; 0.2965 gram gave 0.056 gram Pt = 18.88 per cent. Pt. The expressed mother-waters from the crystals yielded a platinum salt, which gave on ignition—

0.3239 gram = 0.061 gram Pt = 18.83 per cent.

Mean of three determinations = 18.85 per cent. Pt.

$C_{34}H_{36}(C_2H_5)_2NO_6 \cdot PtH_2Cl_6$  = 18.97 per cent. Pt.

$C_{34}H_{37}(C_2H_5)N_2O_6 \cdot PtH_2Cl_6$  = 19.50 per cent. Pt.

Whence it is manifest that under the conditions described, only diethylmorphine is formed (using the nomenclature adopted by Wright). In fine, there does not appear to be any evidence to justify the adoption of a higher formula for morphine than the empirical ( $C_{17}H_{19}NO_3$ ), which is the formula still in general use. It follows that Wright's "diacetylmorphine" should be named *acetylmorphine*, and his "tetracetylmorphine" *diacetylmorphine*.

#### 4. On the Fossil Flora of the Staffordshire Coal Fields.

I. The Fossil Plants collected during the Sinking of the Shaft of the Hamstead Colliery, Great Barr, near Birmingham. By Robert Kidston, F.R.S.E., F.G.S.

#### 5. On a Monochromatic Rainbow. By John Aitken.

A monochromatic rainbow looks like a contradiction in terms. As a rainbow of this kind was, however, seen lately, its occurrence seems worth putting on record. On the afternoon of Christmas day I went for a walk in the direction of the high ground to the south of Falkirk. Shortly after starting I observed in the east

what appeared to be a peculiar pillar-like cloud, lit up with the light of the setting sun. What specially attracted my attention was that the streak of illumination was vertical, and not the usual horizontal band-form we are accustomed to. I looked in the direction of the sun to see if I could trace any peculiar opening in the clouds through which the light passed, but failed to do so.

I continued observing for some time the peculiar appearance, when at last the pillar-like illumination became more elevated, and by the time the sun was just setting and I had arrived on the high ground, it had reached to a considerable height, and I at last began to suspect that what I had been looking at was not a cloud at all, but the "*tooth*" of a rainbow. Soon all doubt was put at rest by the red pillar extending, curving over and forming a perfect arch across the north-east sky.

The rainbow when fully developed was the most extraordinary one I ever saw. There was no colour in it but red; it consisted simply of a red arch, and even the red had a sameness about it; all the other colours were absent. Perhaps this is stating it too strongly, as after careful observation I succeeded in detecting at one or two points traces of yellow; but of green, blue, or violet there was not a vestige, and in their place there was a dark band extending inwards to about the breadth usually occupied by these colours. This band, though distinctly darker than the sky, to the inside of it was not greatly so. Outside the rainbow there was part of a secondary bow, and inside, at certain places, there were indications of a supernumerary bow, as short detached red patches were visible at different points on the inner edge of the dark band.

For some time before the bow developed itself I had been watching the Ochil Hills, which lay to the north of me. These hills at the time were covered with snow, and the setting sun was shining brightly on them. On many occasions I have seen snow-clad hills, in this and other countries, lit up with the light of the setting sun, and glowing with rosy light, but never have I seen such a depth of colour as on this occasion. It was not a rosy red, but a deep furnace red. Now, why was the colour on the hills of so deep a red on this evening? The monochromatic rainbow gives its own explanation; it also tells us why the hills glowed

with so rich a colour. The rainbow is simply nature's spectrum analysis of the sun's light, and it showed that on that occasion the sun's light was shorn of all the rays of short wave lengths on its passage through the atmosphere, and that only the red rays reached the surface of the earth.

But it will be said, that every object on the surface of the earth on that afternoon ought to have appeared red, and nothing but red, if nothing but red rays reached the earth from the sun. Now this was by no means the case. Everything looked not very different from their usual; they appeared simply tinted with red. The reason for this evidently was, that while we received only red light direct from the sun, there was a great deal of green, blue, and violet light reflected from the sky overhead, and these combining with the red caused the light to be but little different from the usual. The reason why the Ochils glowed with so deep a red was owing to their being overhung by a dense curtain of clouds, which screened off the light of the sky. Their illumination was thus principally that of the direct red light of the sun.

6. On *Neuropteris plicata*, Sternb., and *Neuropteris rectinervis*, Kidston. By Robert Kidston, F.R.S.E.

7. Reflex Spinal Scratching Movements in some Vertebrates. By Prof. John Berry Haycraft.

Many, who have kept dogs, are aware that if the skin covering the side of the body be scratched, a dog will move the leg of that side as if itself to scratch the part touched. This fact is known to the physiologist, and a, so-called, scratching centre, to which the sensory impulses are carried, and from which motor impulses to the muscles pass, has been shown to exist in the spinal cord.

I would venture to lay before the Society one or two additional facts in this connection which have been observed by me.

First, as to the sensory area from which this reflex may be initiated. The only sensitive part of the skin in most cases is that covering the lower ribs in about the middle of their course. If, however, the dog be sensitive to irritation, if it has suffered from

vermin, or from any irritative condition, the area is much greater. Practically it includes those parts of the skin to which the hind foot can be approximated. It commences posteriorly at the part which the hind leg can reach, generally 2 or 3 inches in front of the flank, though this will vary according to the size of the animal. It extends forwards to the shoulder, including the whole side of the animal, and even reaches up the side of the neck, and on to the root of the ear. The most sensitive portion is that part which in most dogs alone gives the reflex.

The scratching area is very sharply defined. If in a sensitive animal the skin on the back within half an inch of the middle line be scratched the leg of that side will move. If, however, the skin at a corresponding part of the opposite side be touched, the animal will scratch at once with the other leg. The same observations apply to the scratching areas when they extend ventrally to the middle line. The skin of the flank, of the muzzle, of the fore leg are outside this area, and outside the reach of the hind leg. They are scratched by the teeth or fore leg. As in the case of the pithed frog, if one side of the animal be scratched, and if the leg of that side be forcibly held, scratching movements of the opposite leg may often be observed. These movements may be observed in young puppies, and can readily be called forth in animals which are sound asleep.

I have been unable to get these movements from cats, although the cat tribe is probably related to the dog tribe by common ancestry. In the rabbit, too, I have been unable to observe them. If, however, a guinea-pig be killed by a blow on the back of the neck, and if the skin at the side of the belly be gently tickled, the animal will bring the leg of that side rapidly to the part, and scratch it violently for some time. I have noticed, too, that after an ox has been killed by a blow of the pole-axe, the hind leg will be brought to the side of the body if that part be rubbed. The movement is similar to that made during life to get rid of flies. We see then that these reflex scratchings are sometimes present, sometimes absent in animals nearly related. This variation depends, no doubt, on the habits, but more especially upon the build of the animal itself. The cat possesses great mobility of the head and neck. It can lick its sides, and can reach most parts of its body with its fore



claws. The dog cannot do this, and the hind leg is used instead. The rabbit has a mobile and flexible body, which it cleans in a sitting posture with its mouth and fore paws. The guinea-pig and the ox are shorter, with thick-set necks, and the hind leg is called into requisition.

One cannot attain spinal scratching reflexes from the human subject, and probably not from the apes. The theory of build, and bodily mobility, will not entirely explain these cases, however, for another consideration appears. This we shall now consider.

Co-ordinated reflex movements may be divided into two classes. In the first class we have movements of limbs, the aim of which is to bring them into relationship with other parts of the body. Such are the complex movements of the pithed frog, and the scratching movements we are discussing. In these cases all that is essential for the acquisition of the power of bringing one part of the body into connection with another part is tactile sensibility of the skin. The other classes of co-ordinated reflexes are those which change the position of the body in respect to its surroundings, *e.g.*, walking, swimming, &c. In this case sensation of sight, hearing, &c. are required in addition.

The nerves of tactile sensibility for the trunk and limbs pass to the cord in which they make connection with motor-fibres passing to those parts of the body. The nerves of hearing, sight, &c., pass to the brain.

It follows from this that the first class of movements *may* take place in a pithed animal; the latter, never. Now, one constantly finds the remarks that spinal, co-ordinated movements present in lower animals, *e.g.*, the frog, have their centres in higher regions of the nervous system in higher animals. This and similar remarks indicate, I think, a grave misconception.

The second class of co-ordinated movements are never purely spinal either in the frog or in any other vertebrate. It is probable in those cases of the first class, in which the co-operation of the brain with the spinal cord is necessary, that this is not the result merely of higher development, but depends upon other causes, some of which I have touched upon.

The scratching movements, quite as complex as any of the movements of the pithed frog, require for their performance the cord

alone, both in the case of the guinea-pig and dog. The rabbit and the cat certainly do not possess more highly developed brains, yet no such scratching movements can be elicited. The difference does not depend then upon a question of development either upwards or downwards, but rather upon a variation of habit or build. From increased mobility of the body, or from altered habits, the cat and rabbit may have come to use their eyes and head, whereby the brain is called into action, in place of the leg used by some ancestral type. Or, again, it is possible that the dog and guinea-pig may have acquired the use of the leg for scratching from altered habits, or from loss of mobility.

Nor is it difficult to explain the condition of things seen in the human subject. A child is born without such working connections between the sensory surfaces of its body and the corresponding groups of muscles as would lead to the approximation of a limb to any particular part of the skin. This comes only by laborious experimentation on the part of the child. It sees its foot, and directs its hand to it. It feels the touch, and is conscious of the movement it has made. By continual practice it can touch most parts of its body. This is learnt only by experience, which has involved the use of sight, and therefore depends largely on the action of the brain.

On this account, if the spinal cord be divided, we should not expect a man, upon having the calf of one leg tickled, to be able to scratch it with the foot of the other leg, because during his extra-uterine development the brain was a necessary factor in producing such movements. Such is, indeed, the case, for although spasmodic jerks of muscles may be called forth by stimuli applied to the skin, an absence of purposive movements is noticed as soon as the cord is severed.

### 8. Reply to Professor Boltzmann. By Prof. Tait.

Hearing, again by accident, that Professor Boltzmann has in the Vienna *Sitzungsberichte* published a new attack on my papers about the *Kinetic Theory*, I at once ordered a copy, which has at length arrived. As my papers appeared in our *Transactions*, I think my answer to this fresh attack should be communicated in the first

place to this Society. The time I can spare for such work at this period of the year is very scant, and Prof. Boltzmann has raised a multitude of questions. I will take them in order. But I must commence by saying, with reference to Prof. Boltzmann's peculiar remarks on my behaviour as a critic, that, while leaving them to the judgment of readers, I shall have to bring before the same readers several instances in which Prof. Boltzmann has completely misstated the contents or the objects of my papers. This is not a new departure. In his first attack on me he said that I had nowhere stated that my investigations were confined to hard spherical particles; whereas I had been particularly explicit on that very point. But fresh cases of a similar character abound in this new attack.

*First.* There runs through this paper an undercurrent, at least, of accusation against me for putting forward my results as new, and thus ignoring the work of others. I had no such intention, and I do not think anything I have said can bear such a construction. My knowledge of the later history of the subject is no doubt now considerably greater than it was about two years ago when, at Sir W. Thomson's request, I undertook an examination of Clerk-Maxwell's first proof of his own Theorem. But it is still of a very fragmentary character. I had, years ago, read papers by Maxwell and by Clausius; and had glanced at the treatises of O. E. Meyer and Watson. I had also made a collection of various papers by Prof. Boltzmann. But I found that, without much expenditure of time and labour, it would be impossible to master the contents of the three last-named works, mainly because the methods employed seemed to me altogether unnecessarily intricate. [I have already stated the impression produced on me by such of Prof. Boltzmann's papers as I have tried to read, and I need not recur to it.] I therefore set to work for myself, having certain definite asserted results in view, but little knowledge of the processes which their discoverers or propounders had used. After obtaining a demonstration of Clerk-Maxwell's Theorem, I was led to pursue my investigations into other matters, such as the rate of restoration of the special state, the size of molecules, &c. I brought before the Society such of these investigations as I had more fully developed; and I hope to communicate others. One object which I tried to

keep constantly in view was to make my papers at least *easily intelligible*. Intelligibility is not too common a characteristic of papers or treatises on this subject. But if I have succeeded in putting some parts of the *Foundations* of the Kinetic Theory (for to these alone do my papers profess to extend) in a form which renders them easily apprehended, I shall have done a real service to students of Physical Science. The other object at which I aimed was, of course, the verification of Maxwell's Theorem; and of the extension of it (to all degrees of freedom of complex molecules) which was made by Prof. Boltzmann. Sir William Thomson and myself were, in fact, called to the question by the discrepancies between the observed behaviour of gases and the behaviour which Prof. Boltzmann's Theorem would have led us to expect. To test this excessively general theorem, I determined to examine certain special cases, and (that these might be, however imperfectly, represented by systems of free particles) it was necessary to assume want of freedom for collision, though confessedly as *one* step only. I could not, of course, in this way put limits on the excursions or the admissible speeds for different degrees of freedom.

*Second.* While examining, and seeking to improve, the proof which Clerk-Maxwell originally gave of his Theorem, I found it impossible to begin without the assumption of a certain regularity of distribution of masses and velocities; and of course I sought how to *justify* such an assumption. I was thus led to believe that collisions, not merely of particles of the two kinds with one another but among those of each kind, are absolutely necessary for this justification. Then I saw that, in complex molecules, perfect freedom of collisions of all kinds of "degrees of freedom" could not possibly be secured, and that this might, in part at least, account for the discrepance between Prof. Boltzmann's Theorem and the observed behaviour of gases. I saw also that, for the truth even of Maxwell's Theorem, it was necessary that neither of the two gases should be in an overwhelming majority. Thus these two things, which Prof. Boltzmann now speaks of as "physically less important," are from my point of view vital to the general truth of his Theorem.

Prof. Boltzmann commences his recent paper by citing a "general equation" from the Phil. Mag. of April 1887; and of it he says:—

"Bei Ableitung dieser Gleichung habe ich dort im Übrigen



genau dieselben Voraussetzungen zu Grunde gelegt, welche auch Herr Tait machte, nur dass ich über die relative Grösse der Durchmesser  $\lambda$  und  $\Lambda$  der Molecüle beider Gase, sowie über den Grössenwerth des Verhältnisses  $N_1 : N_2$  nicht die mindeste Annahme gemacht habe."

This is so far from being the case, that it was precisely his *assumptions*, and not his proof, which I disputed. My remark was:—

"I think it will be allowed that Prof. Boltzmann's assumptions, which (it is easy to see) practically beg the whole question, are themselves inadmissible, *except as consequences of the mutual impacts of the particles in each of the two systems separately.*"

Of course, with his assumptions, Prof. Boltzmann obtains the desired result:—having in them virtually begged the question. He now blames me for not having said a word in refutation of his proof, for I had professed my willingness to allow its accuracy without even reading it. There was no discourtesy in that remark:—nothing but a cheerful admission that, in the hands of Prof. Boltzmann, such premises could not fail to give the result sought. My comments were in fact *necessarily* confined to the assumptions. For, as I could not admit them, the proof founded on them had no interest for me. Professor Boltzmann assumed that two sets of particles, *even if they have no internal collisions*, will by their mutual collisions arrive at a state of uniform distribution in space, and of average behaviour alike in all directions. This may possibly be true, but it is certainly very far from being axiomatic, and thus demands strict proof before it can be lawfully used as a basis for further argument. In quoting my remarks on this point Prof. Boltzmann very significantly puts an "&c." in place of the following words:—"it is specially to be noted that this is a question of *effective diameters* only and not of masses:—so that those particles which are virtually free from the self-regulating power of mutual collisions, and therefore form a disturbing element, may be much more massive than the others." It was of this preliminary matter, of course, that I spoke when I wrote the following sentence, which seems to have annoyed Prof. Boltzmann:—

"I have not yet seen any attempt to prove that two sets of particles, which have no internal collisions, will by their mutual collisions tend to the state assumed by Prof. Boltzmann."

I think it probable that Prof. Boltzmann has not fully apprehended the meaning of the word "assumed" in this sentence. Otherwise I cannot understand why he is annoyed because I took his proof for granted.

In taking leave, for the time, of this special question, I need scarcely do more, and I cannot do less, than reaffirm the assertion just quoted:—while adding the remark that this is very far from being my sole objection to Prof. Boltzmann's very general Theorem. In fact Professors Burnside\* and J. J. Thomson† have quite recently advanced other serious objections. Prof. Boltzmann's Theorem, in a word, is not yet demonstrated.

*Third.* As to the questions of viscosity and heat-conduction; my investigations were expressly made on the assumption that change of permeability, due to motion, was negligible. When I found that I had obtained in a very simple way certain characteristic results of Clerk-Maxwell and of Clausius respectively, I was satisfied with the approximation I had made. Prof. Boltzmann does not allude to the fact that my investigation was distinctly stated to be an approximate one only, and that the additional consideration he now adduces had been before me and had been rejected (rightly or wrongly) for reasons given. I said—

"Strictly speaking, the exponent should have had an additional term . . . . . See the remarks in § 39 below."

And, in the § 39 thus pointedly referred to, one of the remarks in question is—

"We neglect, however, as insensible the difference between the absorption due to *slowly* moving layers and that due to the same when stationary."

And, in fact, the result which I gave for the viscosity (and which Professor Boltzmann, without doubt justly, claims as his own) is correct under the conditions by which I restricted my investigation. The introduction of the consideration of change of permeability due to the shearing motion involves an alteration of about eleven or twelve per cent. only in this *avowedly approximate* result. Of this I have assured myself by a rough calculation, and I will work it out more fully when I have leisure. It seems that I missed this in looking over Meyer's book, and, according to Prof. Boltzmann,

\* *Trans. R.S.E.*, 1887.

† *Phil. Trans.*, 1887.

all investigators except Meyer have fallen into the same trap. Meanwhile the calculation with which Professor Boltzmann has furnished me gives an excellent example of his style, for it is altogether unnecessarily tedious. And it seems to contain two gigantic errors which, however, compensate one another. For his integrand contains the factor  $e^{-xf/p}$ . Here  $f$  is a signless quantity, and the limits show that  $x$  is always positive and  $p$  always negative. As written, therefore, the integral is infinite, though in the result it is made to come out finite. The object of the paragraphs 1 and 2, which immediately follow, is unintelligible to me. The former seems to suggest the use of an unsound method, the latter has no discoverable bearing on anything that I have written. Prof. Boltzmann has also afforded an idea of the value which he himself attaches to the terrific array of symbols in the 95 pages of his 1881 paper (to which he refers me) by now allowing that he is not prepared to assert that any one of three determinations of the coefficient of viscosity which he quotes (mine, or rather his own, being among them) is to be preferred to the others!

*Fourth.* Prof. Boltzmann refers to my remarks on Mr Burbury's assertion that a single particle, with which they can collide, would reduce to the special state a group of non-colliding particles. Prof. Boltzmann signified his belief in the truth of this proposition; and in answer I showed that (were it true) æons would be required for the process, even if that were limited to a single cubic inch of gas. He now calls this an "entirely new question" and will not "prolong the controversy by its discussion." I do not see that, so far at least as the "controversy" is concerned, it is any newer than the rest. It is contained in the first instalment of his attack. Why then should he now desert it? But Prof. Boltzmann, in thus leaving the subject, takes a step well calculated to prolong the discussion, for he represents me as speaking of the instantaneous reversal of the motions of *all* the particles, whereas my argument was specially based on the reversal of the motion of the single stranger alone, a contingency which might possibly occur by collision even with a particle of the gas, certainly by collision with the containing vessel. There is a common proverb, "All roads lead to Rome." It seems it ought now to be amended by the addition, "whether you go backwards or forwards along them."

*Fifth.* As to my proof (so designated) of the Maxwell Law of distribution of velocities :—I have already explained that this part of my paper was a mere introductory sketch, intended to make into a connected whole a series of detached investigations, and therefore contained no detailed and formal proofs whatever. Maxwell's result as to the error-law distribution of velocities, being universally accepted, was thus discussed in the briefest manner possible. I said also that a detailed proof can be given on the lines of § 21 of my paper. Prof. Boltzmann\* at first accused me of reasoning in a *circulus vitiosus*, and went the extreme length of asserting that the independence of velocities in different directions can do no more than prove the density (in the velocity space diagram) to be dependent on the radius vector only. Now, when I have taken the trouble to point out briefly and without detail what I meant by the statements he misunderstood, he says I have admitted that my proof is defective ! For my own part, I see no strong reason wholly to reject even the first proof given by Maxwell ; and it must be observed that although its author said (in 1866) that it depends on an assumption which " may appear precarious," this did not necessarily imply that it appeared to *himself* to be precarious. The question really at issue was raised in a very clear form by Prof. Newcomb, who was the earliest to take exception to my first sketch of a proof. He remarked that it seemed to him to possess too much of a geometrical character (*i.e.* to prove a physical statement by mere space-reasoning), while Maxwell's seemed to involve an unauthorized application of the Theory of Probabilities. In consequence of this objection I examined the question from a great many points of view, but I still think my original statement correct. What I said was "*But the argument above shows* further, that this density must be expressible in the form

$$f(x) f(y) f(z)$$

whatever rectangular axes be chosen passing through the origin." In my second paper I said (in explanation of this to Prof. Boltzmann), that the behaviour parallel to  $y$  and  $z$  (though not the

\* This addition to Prof. Boltzmann's first attack on me seems to have appeared in the *Phil. Mag.* alone. It is not in either of the German copies in my possession (for one of which I am indebted to the author), nor do I find it in the *Sitzungsberichte* of the Vienna Academy



number) of particles whose velocity components are from  $x$  to  $x + dx$ , must *obviously* be independent of  $x$ , so that the density of "ends" in the velocity space diagram is of the form  $f(x) \cdot F(y, z)$ . The word I have underlined may be very easily justified. No collisions count, except those in which the line of centres is practically perpendicular to  $x$  (for the others each dismiss a particle from the minority; and its place is instantly supplied by another, which behaves exactly as the first did), and therefore the component of the relative speed *involved in the collisions which we require to consider* depends wholly on  $y$  and  $z$  motions. Also, for the same reason, the frequency of collisions of various kinds (so far as  $x$  is concerned) does not come into question. Thus the  $y$  and  $z$  speeds, not only in one  $x$  layer but in all, are entirely independent of  $x$ ; though the *number* of particles in the layer depends on  $x$  alone. Prof. Boltzmann's remark about my quotation from De Morgan will now be seen to be somewhat irrelevant so far as I am concerned, though he may (perhaps justly) apply it to some of his own work.

*Sixth.* As to the Mean Path, though I still hold my own definition to be the correct one, I would for the present merely say that Professor Boltzmann entirely avoids the statement I made to the effect that those who adopt Maxwell's definition, which is not the ordinary definition of a "mean," must face the question "Why not . . . . . define the mean path as the product of the average speed into the average time of describing a free path?" The matter is, however, of so little moment, that a very great authority, whom I consulted as to the correct definition of the Mean Free Path, told me that the preferable one was that which lent itself most readily to integration.

*Seventh.* In his remarks upon the effect of external potential, Prof. Boltzmann does not defend his proof to which I objected, but gives a new and fearfully elaborate one. And he quotes, as a remark of mine on this entirely different proof, the phrase "this remarkable procedure" which I had applied to his objectionable old one! He also treats in a disparaging manner the assumption on which my very short investigation is based; viz. "*When a system of colliding particles has reached its final state, we may assume that (on the average) for every particle which enters, and undergoes collision in, a thin layer, another goes out from the other*

*side of the layer precisely as the first would have done had it escaped collision.*" Of course it would be easy to make a 20 page proof of this by the help of an imposing array of multiple integrals. But this would be the sort of thing which I have called "playing with symbols," i.e. using them *instead of thought*, while their proper function is to *assist thought*. A mathematical demonstration does not *necessarily* imply the use of symbols, any more than that of diagrams:—and, when we find an author continually using symbols to establish what is obvious without them, we very naturally question the validity of his symbolical processes when they are employed for their legitimate purpose. I still think the assumption above a legitimate and indeed almost an obvious one; but it is strange that an objection of this kind should come from a writer like Prof. Boltzmann, who (see head *Second* above) has made, and still defends, a fundamental assumption (of the class to which he applies the term "unbewiesene Voraussetzung") which most clamantly demands proof.

*Finally*, as Prof. Boltzmann objects alike to Greek, and to English, quotations, although they have Plato and De Morgan for their authors, what does he say to the Latin one

*"Quis tulerit Gracchos de seditione querentes"?*

#### PRIVATE BUSINESS.

Rudolph Julius Emmanuel Clausius, Professor of Natural Philosophy in the University of Bonn; Ernest Haeckel, Professor of Zoology and Histology in the University of Jena; Demetrius Ivanovich Mendeléff, Professor of Chemistry in the University of St Petersburg, who had been proposed as Foreign Honorary Fellows, and had been named from the Chair in terms of Law XII., at the Meeting of 5th December 1887, were balloted for, and declared duly elected Foreign Honorary Fellows of the Society.

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*Monday, February 6, 1888.*

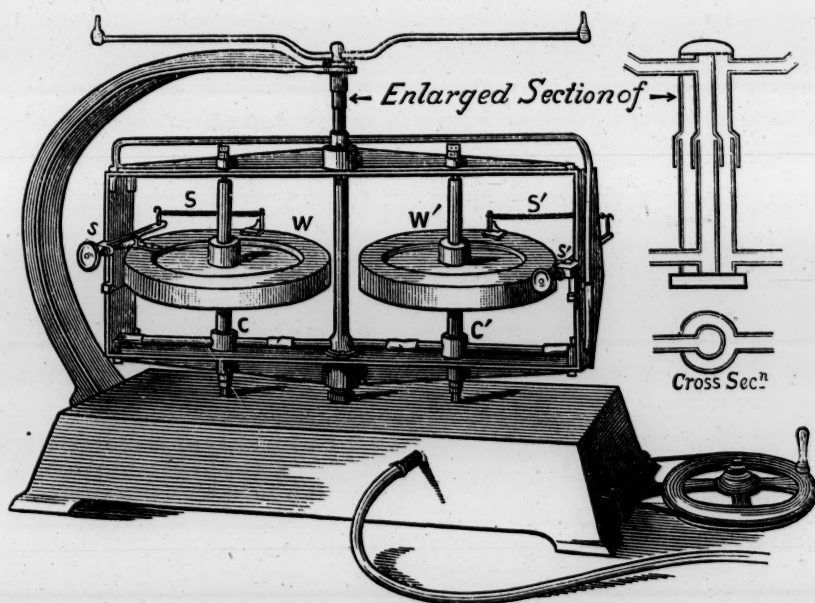
SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On a Mode of Exhibiting the Action of the Semi-circular Canals of the Internal Ear. By Professor Crum Brown.

The problem is to contrive an apparatus in which, by means of inertia, acceleration of angular velocity about any axis in either sense may be observed and approximately measured.

In the internal ear we have such an apparatus, if we consider the two ears with their six canals as forming one organ, each canal sensitive to acceleration in one sense about the axis at right angles to the average plane of the canal. The model shown illustrates the



principle. It consists of a rectangular frame turning on a vertical axis parallel to the short sides of the rectangle. On each side of this axis there is a heavy wheel with its axle vertical. Each wheel

is fitted with a stop which prevents its rotating, relatively to the frame, beyond a certain position (which we may call its normal position) in one sense. One wheel, *W*, cannot rotate beyond its normal position in the positive sense, the other, *W'*, cannot rotate beyond its normal position in the negative sense. *W* can rotate negatively, but in doing so stretches a spring, and the spring is made strong enough to prevent any but a very slight angular movement, with the greatest acceleration to which the instrument can be exposed. Similarly *W'* can rotate positively, but in doing so stretches a spring; in fact, *W* and *W'* are mirror images of each other, the two springs *S* and *S'* being as nearly as possible equal. If, now, the frame receives an acceleration of positive rotation, the two wheels tend to rotate negatively, relatively to the frame; but *W'* cannot do so, it is forced by its stop to rotate with the frame. But *W* does rotate relatively to the frame and stretches its spring. The extent to which the spring is stretched is approximately a measure of the acceleration. If we could keep the acceleration constant, the wheel would remain at the same angular distance from its normal position with its spring stretched. But if we make the acceleration zero, *i.e.*, make the rotation uniform, the spring brings back the wheel to its normal position. What is true of *W* and *W'* with positive acceleration, is of course true of *W'* and *W* with negative acceleration. While the frame is rotating, we cannot easily see whether the wheels are in their normal position or not, or how far they have rotated from them. It is necessary, therefore, to contrive some way of indicating this. In the model shown this is done by leading gas through the lower part of the axis of the frame, and by two pipes, one to each wheel. On the axle of each wheel, where the gas-pipe passes it, there is a stop-cock. In the normal position this stop-cock is nearly closed, so as to allow only a little gas to pass; as the wheel rotates away from its normal position the stop-cock opens. From the stop-cocks the gas-pipes pass round to the upper part of the axis of the frame, and pass out through it through a joint to two fixed gas jets. Acceleration in the positive sense opens the stop-cock of *W*, and the corresponding gas jet flares up. Acceleration in the negative sense opens the stop-cock of *W'*, and its jet flares up. When the rotation of the frame is uniform, whatever its rate may be, the wheels remain in their



normal position, and the gas jets at their minimum. When this rate changes, we have acceleration in the one or other sense, and this is indicated by the flaring up of the corresponding gas jet.

The model illustrates merely one-third of the complete apparatus, as it shows the results of acceleration about one axis only, and in both senses about that axis.

Perhaps, in making such a model, it would be better to work the stop-cocks by means of cranks from the wheels, and so diminish the friction of the axles on their bearings. However, in the model shown, this friction is so small that very moderate acceleration is well indicated.

The apparatus was made, from Professor Crum Brown's instructions, by Mr Alexander Frazer, 7 Lothian Street, Edinburgh.

2. On the Temperature and Currents in the Lochs of the West of Scotland, as affected by Winds. By John Murray, Esq.

3. Note on the Influence of Pressure on the Solubility of Carbonate of Lime in Sea Water containing Free Carbonic Acid. By W. G. Reid. *Communicated by* JOHN MURRAY, Esq.

Analysis of the dredgings brought to the surface during the Voyage of H.M.S. "Challenger," has shown, that in deeper water as the depth increased, the quantity of carbonate of lime shells decreased,\* and as the pressure is in direct proportion to the depth under water, it was surmised that some connection existed between the pressure and the disappearance of lime shells. To ascertain if there was any truth in this surmise, Mr Murray suggested the following experiments. The results are unfortunately incomplete; nevertheless, Mr Murray thinks it advisable to publish them.

During this investigation, I had the honour of working with Mr H. N. Dickson, who, with his hands full of more important work,

\* Murray, "On Coral Reefs," *Proc. Roy. Soc. Edin.*, 1880, p. 509; and *Narrative of the Cruise of the Challenger*, p. 923.

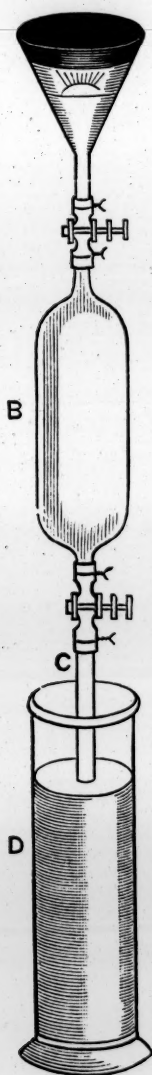
superintended the physical part of these experiments with characteristic patience and kindness.

This subject does not seem to have been previously investigated. Th. Schloesing and others have demonstrated that the solubility of carbonate of lime, &c., in water containing carbonic acid, increases as the pressure of carbonic acid increases, and that

according to a definite law; but nothing is said about the effect on the solubility, when, the quantity of carbonic acid in solution remaining the same, the pressure is increased. The latter was the object aimed at in these experiments, and as they had special reference to the conditions existing in the ocean, sea water was taken and charged with a definite quantity of carbonic acid, that the effect might be exaggerated, and therefore more easily studied.

For the experiments done under pressure the following *modus operandi* was adopted:—

A (see sketch) is a Bohemian glass funnel, having a glass cover ground to fit; a flat india-rubber band of the same circumference as the cover is put between it and the funnel. A rubber capsule is now stretched over the top, and for greater security the neck of the funnel is passed through a slit in a strong rubber band, which is then stretched over the capsule. The funnel contains a filter paper, and a cambric bag, within which is a weighed quantity of shells. To the funnel the bulb B is attached, and this is connected by the glass tube C to a vessel containing mercury D. The whole is tied to a suitable support, and is ready for immersion in the water, contained



in the pressure apparatus, *i.e.*, the celebrated "gun" belonging to the "Challenger" Commission. The bulb B, which has been accurately measured, contains sea water charged in the following manner with a definite quantity of carbon dioxide. B is first filled with sea

water, noting the temperature, and the pieces of india-rubber tubing at each end securely clamped. The tube C is attached and secured with wire, then filled with mercury, and another bulb, also accurately measured, and filled with carbon dioxide at a known temperature and pressure, is attached in the same way as B, to the other end of the tube. The clamps are now unscrewed, and the carbon dioxide is allowed to come in contact with the water, and as it is absorbed the vacancy so caused is filled up by allowing mercury to be sucked in. When the carbon dioxide is all dissolved, the mercury is allowed to flow to the smaller bulb (always that which contained the carbon dioxide), which may be detached after clamping the rubber tube at the bottom of tube C. Where pressure is applied, the mercury is forced up into the bulb, and the water into the funnel, the air in which at the pressure employed (4 tons per square inch, or nearly 600 atmospheres) contracts to a very small bubble, and thus allows the water to come into contact with the shells. In these experiments the pressure was kept at its maximum for 30 minutes, then released, and again applied and kept up for 60 minutes. When the pressure is let off, the air expands and drives the water out of the funnel; the effect therefore of the break in the application of the pressure is to cause a slight agitation, and to bring a fresh portion of the liquid into contact with the shells. After each experiment the alkalinity of the water was carefully determined, and the original alkalinity deducted therefrom: the quantity of carbonate of lime dissolved was thus ascertained.

Each of the experiments made under pressure was repeated, at the ordinary pressure, under as nearly as possible the same conditions. The funnel taken in this case was larger, and a scratch on it indicated the capacity of the funnel used for the corresponding pressure experiment. A glass cover protected the contents of the funnel from dust. The lower end of the bulb B was attached to a tube which passed through an india-rubber stopper in one neck of a small Woolff's bottle, through the other neck a funnel tube about 20 inches long was passed. Both tubes dipped under the surface of mercury contained by the Woolff's bottle, and by pouring mercury into the funnel tube, the water was forced into the funnel until it reached the scratch aforementioned.

Table showing Influence of Pressure on the Solubility of Carbonate of Lime in Sea Water containing Carbonic Acid.

No. of Experiment.	Carbonate of Lime used.		Quantity of Sea Water taken.			Carbon Dioxide.				Pressure.		Temperature.	Resulting Alkalinity per Litre.	Res. Alk. (m) minus original Alkalinity.	(n) = $\text{CaCO}_3$ dissolved per Litre Sea Water.	(p) $\text{CaCO}_3$ dissolved per grm. $\text{CO}_2$ taken.
	Capacity of Funnel.	Kind.	Quantity.	(d)	(e)	Taken.	Vol.	Weight.	Per Litre Sea Water.	Amount.	Time at Maximum.					
(a)	(b)	(c)	(d)	(d)	(e)	Vol.	(f)	(g)	(h)	(i)	(k)	(l)	(m)	(n)	(o)	(p)
I.	c.c.	Glob. I.	grms.	c.c.	c.c.	grms.	c.c.	grms.	grms.	4 tons.	minutes.	12.0	91.5	39.9	grms.	grms.
II.	65.5	Do.	1.5092	106.65	54.33	.1074	509.4	.1071	1.0494	4 "	30 to 60	12.0	117.0	65.4	.0906	.0904
III.	84.4	Do.	1.5867	117.53	62.39	.1233	530.8	.1237	1.0523	4 "	Do.	12.0	104.0	65.4	.1486	.1416
IV.	65.5	Do.	1.5897	117.53	62.56	.1237	532.3	.1237	1.0523	4 "	Do.	9.0	104.0	47.5	.1191	.1106
V.	65.5	Do.	1.5800	106.65	55.04	.1088	516.1	.1088	1.0203	4 "	Do.	9.0	98.1	47.5	.1080	.1060
VI.	84.4	Do.	1.5020	106.65	54.50	.1077	511.0	.1077	1.0102	Atmospheric.	Do.	12.2	76.6	25.0	.0568	.0562
VII.	84.4	Do.	1.5620	117.54	63.84	.1263	543.4	.1263	1.0744	Do.	Do.	10.0	76.8	25.2	.0572	.0545
VIII.	91.1	Do.	1.5620	117.53	64.06	.1266	545.0	.1277	1.0777	2 tons.	Do.	8.3	95.6	44.0	.0999	.0927
IX.	65.5	Glob. II.	1.5030	106.65	55.43	.1096	519.7	.10275	1.0275	4 "	Do.	8.6	85.1	33.5	.0761	.0758
X.	84.4	Do.	1.5610	117.54	63.42	.1254	539.6	.1254	1.0668	4 "	Do.	8.8	95.5	43.9	.0997	.0935
XI.	65.5	Do.	1.5020	106.6	53.92	.1066	505.9	.1001	1.0001	Atmospheric.	Do.	15.0	61.9	10.4	.0236	.0236
XII.	84.4	Do.	1.5660	117.5	59.67	.1180	507.8	.1039	1.0039	Do.	Do.	13.2	63.4	11.9	.0270	.0269
XIII.	91.1	Coral sand I.	1.5580	117.54	63.40	.1256	539.4	.1064	1.0664	4 tons.	Do.	8.8	113.1	61.5	.1398	.1311
XIV.	65.5	Do.	1.5025	106.65	54.57	.1079	511.4	.1015	1.0115	4 "	Do.	8.8	96.0	44.4	.1011	.0999
XV.	91.1	Do.	1.5370	117.5	61.82	.1222	526.2	.10402	1.0402	Atmospheric.	Do.	11.6	71.4	18.9	.0429	.0413
XVI.	65.5	Do.	1.5020	106.6	54.29	.1073	509.3	.10069	1.0069	Do.	Do.	10.5	70.9	18.4	.0418	.0425
XVII.	91.1	Do.	1.5380	117.5	57.45	.1135	488.9	.9666	1.0066	Do.	Do.	12.2	69.42	17.92	.0407	.0421
XVIII.	91.1	Coral sand II.	1.5395	117.54	63.04	.1246	536.0	.10396	1.0396	4 tons.	Do.	9.1	105.3	53.7	.1220	.1152
XIX.	91.1	Do.	1.5600	117.53	63.81	.1261	542.9	.10738	1.0738	Atmospheric.	Do.	9.0	68.8	17.2	.0391	.0364
XX.	91.1	Pteropods.	.9850	117.54	64.08	.1267	545.2	.10779	1.0779	4 tons.	Do.	8.3	99.9	48.3	.1098	.1019
XXI.	91.1	Crystal.	3.0027	117.52	62.76	.1244	533.9	.10357	1.0357	Atmospheric.	Do.	11.0	74.3	22.7	.0516	.0490
XXII.	91.1	Do.	...	117.52	62.11	.1211	521.4	.10307	1.0307	4 tons.	Do.	8.3	67.9	16.3	.0371	.0360
XXIII.	91.1	Do.	...	106.65	53.22	.1052	499.0	.9865	1.0365	4 "	Do.	9.0	57.4	5.8	.0132	.0137
XXIV.	91.1	Do.	3.1387	117.48	63.12	.1050	498.0	.9845	1.0345	Atmospheric.	Do.	10.0	56.1	4.5	.0102	.0104
XXV.	91.1	Do.	3.1379	117.53	62.56	.1223	526.5	.10410	1.0410	Do.	Do.	12.2	55.1	3.6	.0082	.0078
XXVI.	91.1	Do.	3.1371	117.53	62.50	.1235	531.7	.10513	1.0513	Do.	Do.	12.2	53.7	2.2	.0050	.0047
XXVII.	91.1	Do.	3.1371	117.49	62.04	.1226	528.0	.10440	1.0440	Do.	Do.	13.1	53.0	1.5	.0034	.0032
XXVIII.	91.1	Do.	3.1355	117.55	62.62	.1238	532.7	.10532	1.0532	Do.	Do.	14.7	53.6	2.1	.0048	.0045
XXVIII.	91.1	Do. ground.	2.9938	117.5	62.62	.1238	532.9	.10536	1.0536	Do.	Do.	12.6	65.7	14.2	.0322	.0306



The accompanying table gives the results of the experiments. The columns of the table are explained by the headings, and Mr Murray adds the following notes with reference to the contractions used in column (c).

GLOBIGERINA OOZE.—Collected on the 21st March 1876, in the South Atlantic. Lat.  $21^{\circ} 15' S.$ ; long.  $14^{\circ} 2' W.$ ; depth, 1990 fathoms.

*Specimen I.*, consists of the larger shells in this deposit, such as the shells of the Pelagic Globigermidæ, such as *Orbulina universa*, *Globigerina hustigernia*, *Spheroidina*, *Pullenia*, *Pulvinulina*. In addition to these there were the shells of a few bottom living Foraminifera, as *Biloculina*, and fragments of Echinoderms, Lamellibranchs, and otoliths of fish. The average size of the shells and particles in this specimen is about  $\cdot 6$  of a millimetre.

*Specimen II.*, consists of the smallest shells in the same deposit, being almost wholly made up of young shells of the above mentioned Globigermidæ. The average diameter of the grains of this fine sand are less than  $\cdot 1$  of a millimetre.

CORAL SAND.—Collected off the Great Barrier Reef of Australia on the 31st August 1874. Lat.  $11^{\circ} 35' 25'' S.$ ; long.  $144^{\circ} 2' E.$ ; depth, 135 fathoms.

*Sample I.*, consists chiefly of the coarser fragments of these deposits, and is made up of particles of broken Pteropods, Gasteropods, Lamellibranchs, Echinoderms, Polyzoa, Serpulæ tubes, and numerous Foraminifera. The average size of the fragments were from 2 to 3 millimetres in diameters.

*Sample II.* This was a sample from the same deposit, and made up of the same kind of fragments as Sample I., but these were considerably smaller in size.

PTEROPODS.—These consisted of the shells of *Cavolinia clio*, *Cuvierina*, *Limacina*, and shells of *Atlanta*. These were complete, or nearly complete shells, and apparently free from sand and mud, and were picked out from the coral sand above mentioned.

The last column in the table (column *p*) contains the results stated, so as to render all the experiments comparable. Taking these figures, we have the following average results :—

	Amount of CaCO <sub>3</sub> dissolved per grm., CO <sub>2</sub> taken.	Difference from Extremes.
<i>Globigerina Ooze, I.</i>		
At 4 tons pressure, . . .	·1121	± ·03
At 2 „ „ . . .	·1019	
At atmospheric pressure, .	·0553	± ·0009
<i>Globigerina Ooze, II.</i>		
At 4 tons pressure, . . .	·0846	± ·009
At atmospheric pressure, .	·0252	± ·0017
<i>Coral Sand, I.</i>		
At 4 tons pressure, . . .	·1155	± ·015
At atmospheric pressure, .	·0419	± ·0006
<i>Pteropods.</i>		
At 4 tons pressure, . . .	·1018	
<i>Crystal of Iceland Spar (XXIV.-XXVII.).</i>		
At atmospheric pressure, .	·0050	± ·0018
<i>Crystal, ground to coarse powder.</i>		
At atmospheric pressure, .	·0322	

The disparity between the various results obtained in the pressure experiments I am unable to account for satisfactorily. Nevertheless, the amount of carbonate of lime dissolved at a pressure of 4 tons per square inch, is so much greater than the amount dissolved at the ordinary pressure, that I think it justifies the conclusion that the effect of pressure is to increase the rate of solution; or, in other words, that the chemical activity of a solution of carbonic acid is increased by pressure.

It is to be noted, that although these results may indicate that the solution of carbonate of lime in carbonic acid water is more rapid under high pressures, it by no means follows that the solubility is *greater* than at the ordinary pressure (*ceteris paribus*). Schloesing and other investigators have shown, that in order to get

the maximum amount of carbonate of lime dissolved, the carbonic acid solution had to be left in contact, and agitated with the carbonate for five or six days. With the apparatus at our command we could not accomplish this, and had to rest contented with the results given.

In the experiments XX. to XXIII., a crystal of Iceland spar was taken. The results show a gradual falling off in the quantity dissolved. The reason for this I cannot explain, but that it is not due to the properties of Iceland spar is shown by the experiments XXIV. to XXVII. For these another crystal was taken, and after each experiment it was washed, dried, and weighed carefully. The amount of carbonate dissolved by 117·5 c.c. of sea water (the total quantity taken for each experiment) was as under. A is the amount obtained by titration (alkalinity), and W the loss as observed by weighing. Considering the smallness of the quantity to be measured, and the opportunities for observational error, the results agree fairly well with each other.

		A	W
XXIV.	. .	·0009 grms.	·0008 grms.
XXV.	. .	·0006 „	·0008 „
XXVI.	. .	·0004 „	·0010 „
XXVII.	. .	·0006 „	·0007 „

For the last experiment (XXVII.) the crystal, used in the preceding four, was ground to a powder, the grains of which varied from about 1 mm. square down to impalpability. This was done to try the effect of increasing the surface exposed. As was expected, the amount dissolved was much greater (six times).

My thanks are due to Mr T. Lindsay for kind assistance in some of these experiments.

4. On the Distribution of Carbonate of Lime on the Floor, and in the Waters of the Ocean. By John Murray, Esq. (*With Lantern Illustrations.*)
5. On the Number of Dust Particles in the Atmosphere. By John Aitken, Esq.

PRIVATE BUSINESS.

Mr James Mactear, Mr John M'Arthur, Mr Charles A. Fawsitt, Mr George Brook, Professor W. H. Perkin, Mr H. N. Dickson, Mr David Prain, Mr George Muirhead, and Mr Cathcart W. Methven were balloted for, and declared duly elected Fellows of the Society.